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SCIENCE PROGRESS

THE PROGRESS OF THE ELECTRIC LIGHT

By MAURICE SOLOMON, A.C.G.I., M.I.E.E.

Electric lamps have been improved so materially during the past few years that to-day the position of the electric light is radically different from that of ten or even five years ago. At that time the methods of lighting by electricity appeared to be in a stable, if not in a stagnant, state. The carbon filament lamp, the original glow-lamp of Edison and Swan, had been brought to perfection so far as that desirable condition is ever likely to be reached in the case of an article manufactured in many millions annually: for interior lighting of all but the largest rooms it represented the best lamp that the electrical engineer could produce to compete with the high-efficiency mantle at the disposal of the gas engineer. In many respects doubtless a most efficient competitor, it was heavily handicapped by the high running cost. The best that could be hoped for with any confidence was a steady advancement in its use as more and more persons found themselves in a position to indulge in what was undoubtedly a luxury in artificial lighting; any great advance such as would be caused by the opening up of new fields could only be the result of some radical improvement which would at one stroke bring electric lighting into effective competition with gas on the score of cost. That improvement came with the invention of the metallic filament lamp and when once the initial difficulties of commercial manufacture were overcome interior electric lighting was given a fresh start. Almost at the same moment the production of satisfactory flame arc lamp carbons enabled electricians to make a great stride forward in the field of exterior lighting and the lighting of large interiors.

Without wishing in any way to suggest that finality has been reached in the case either of the metallic filament lamp or

of the flame arc, progress at the present time is certainly at a very much less rapid rate than it was, say, five years ago. It is usually so with inventions of this kind. At first there is a period during which the invention is being worked out in the laboratory (generally, it may be noted with regret, the laboratory of some German manufacturer), when the public naturally knows nothing of what is going on: then comes the announcement of the invention and its first display in a more or less crude commercial form, this being followed by a time of rapid progress when almost each week brings forth rumours of fresh discoveries far outstripping those already published. For a time, even the professional public scarcely knows which way to turn or whether the brilliant achievement of to-day may not be eclipsed by the still more brilliant achievement of to-morrow; but before long the goats are separated from the sheep, principles and methods of manufacture which are really on a sound basis begin to emerge clearly and the new position is seen in its true light; extravagant hopes fade and with them fade extravagant fears; in their place arises a genuine conviction of the true scope of the new invention and an earnest endeavour to gather its fruits to the full. Such a period has been reached in the case of the two new types of electric lamp; the present is therefore a suitable opportunity for a general survey of the position.

The glow-lamp or incandescent electric lamp in which the filament is of carbon was invented in 1878 and during twenty years was the only type of electric lamp suitable for interior lighting. During that period it was brought to a high state of perfection and was made suitable for a large range of voltage and candle power. Carbon would indeed seem to be an ideal material for the filament of an electric lamp, as it remains solid at the highest temperature that can be produced: unfortunately, however, the carbon filament in a lamp undergoes changes which prevent it from being used at too high a temperature. The higher the temperature at which it is run the shorter its "life," whether the life be counted as the actual life until the filament fails or as the "economical life" until it is cheaper to discard the lamp and substitute a new one: for such a point usually arises because the lamp deteriorates in use, giving gradually less light though consuming the same or even more energy, so that there comes a time at which the

extra cost of producing the light is so considerable that it more than balances the cost of a new lamp. The precise temperature (or the watts per candle[1]) at which a lamp should be used to give the most economical result depends on the price at which the lamp can be bought and the cost of energy; it can be calculated easily when these particulars are known. But commercial usage strikes a rough average of scientific calculations and it is found in practice that for general purposes carbon filament lamps should consume between 3 and 4 watts per candle when new: under such circumstances they will have a life of about 1,000 hours and an average energy consumption throughout their life at the rate of from 4 to 4·5 watts per candle.

Such then was the best which the electrical engineer could produce up till 1898, when altogether fresh possibilities were made obvious by the invention of the Nernst lamp; this at one stroke practically halved the power consumption per candle. The Nernst lamp was a radical improvement on the carbon filament, more radical indeed in many respects than the metallic filament lamps which have succeeded and eclipsed it. Instead of a filament of a material electrically conductive at all temperatures, Nernst used one made of a mixture of oxides which would ordinarily be classed as non-conducting but which at moderately high temperatures became conducting. This necessitated the preliminary heating of the filament; but once its temperature had been raised to about a red heat the heating effect of the current itself sufficed to maintain the filament in a conducting state. The filament is not affected by the air, so is not enclosed in an exhausted bulb. The Nernst lamp can be started easily by heating the filament with a match; being

[1] The candle power of a light source is the measure of the intensity with which the source radiates light in a given direction compared with the intensity of a standard source—the candle. The value of the standard candle varies slightly in different countries, no international standard having been as yet generally adopted. In this article the English standard candle is taken as unit. The watts consumed by a lamp measure the electric power: one ampere of current flowing under a pressure of one volt is equal to one watt: under such conditions in an hour one watt-hour of energy is consumed. The efficiency of a given electric-light source is therefore measured by the number of candles given per watt. It is, however, general to compare lamps by the inverse function—the number of watts per candle—and this is often wrongly spoken of as the efficiency, though it is really a measure of the inefficiency. The actual temperature of the filament being hard to determine, the watts per candle serve as a useful basis for comparison, being of course lower the higher the temperature.

accustomed, however, to switching on the electric light and reckoning the convenience of the lamps in this respect one of their chief advantages, the public naturally asked for an automatic means of starting the light. This is ingeniously provided by surrounding the filament with an open wound porcelain spiral itself closely wound with fine platinum wire: on switching the lamp into circuit, the current at first flows through this "heater" spiral, raising it to a bright red heat; the filament being now heated by radiation and convection and, in twenty or thirty seconds is so hot that it becomes conducting, and the current then flows through it. In series with the filament is the coil of a small electromagnet actuating an armature which breaks the heater circuit as soon as the current is flowing through the filament. Additional complication is introduced, however, by the fact that, like all pyro-electric conductors, the filament falls in resistance the hotter it gets. In consequence the current is unstable, a slight rise in potential difference between the ends of the filament producing an indefinite increase in current which continues to rise until the filament fuses. To check the rise it is necessary to run the filament in series with a resistance; for this purpose a most ingenious form of resistance has been invented, which consists of a fine iron wire in a vacuum, like a small glow-lamp with an iron wire filament. This steadying resistance has a characteristic markedly opposite to that of the filament itself, the resistance of the iron wire at a dull red heat increasing very rapidly with a slight increase of temperature due to a slight increase of current. In combination with this special resistance the Nernst filament is a perfectly stable conductor.

These two complications had a very unfavourable effect on the success of the lamp. Of a candle-power range which made its use possible in competition with the carbon filament lamp in cases in which its much higher efficiency must have ensured success, the steadying resistance, the heater and the magnetic cut-out converted the Nernst lamp into a sort of miniature arc lamp. The small user was chary of adopting so complicated a mechanism having so many parts liable to get out of order and calling for almost expert knowledge for their maintenance; the large user preferred the more efficient arc lamp, the mechanism of the individual lamp being scarcely more complicated than that of the Nernst lamp and much less complicated for equivalent candle power, as one arc lamp is equivalent to three or

four Nernst lamps. In spite of these drawbacks, the Nernst lamp was a moderate success during several years and might have come into far more general use but for the invention of the metallic filament lamps; perhaps the best that can now be said for the Nernst lamp is that its production undoubtedly stimulated the investigations which led up to the invention of the metallic filament lamps.

The earliest of the metallic filament lamps was the osmium lamp invented and developed by Dr. Auer von Welsbach during the years 1898 to 1903 and first introduced into this country in 1905. Little need be said in reference to this lamp, as it is now no longer in commercial use and was never a great success. It is interesting chiefly as the forerunner of the tungsten lamps, which are manufactured by methods similar to those first tried with the osmium lamp and in some cases gradually developed from those methods. Osmium, however, proved unsuitable as a filament, mainly on account of its extreme brittleness when cold; it is markedly inferior in many respects to tungsten, which has superseded it.

At about the time when the osmium lamp was introduced, Messrs. Siemens & Halske brought out the tantalum lamp, which they had spent several years in developing. This was the first metallic filament lamp with which any genuine success was achieved. The filament of this lamp is made of tantalum prepared by a special process, fused electrically in vacuo and then drawn into a very fine wire: tantalum being of low specific resistance compared with carbon, a very much greater length of filament is needed for a given voltage and to accommodate this length in the lamp bulb it is wound to and fro over a wire support. As the tantalum filament may be heated by a much higher temperature than a carbon filament the lamp is consequently far more efficient. The average consumption in watts per candle is slightly under 2 for direct current lamps—in fact, 1·85 represents a fair average of the trustworthy tests which have been published. The alternating lamp is not quite so efficient; its consumption may be taken as 2 watts per candle or a little over. It will be seen therefore that the invention of this lamp halved the cost of energy for electric lighting: as the lamp was in all other respects a thoroughly commercial article, it is not surprising that its success was immediate.

In comparison with the Nernst lamp, there could be claimed for the tantalum lamp the same or even greater efficiency—a useful life of 1,000 hours, equal to that of carbon filament lamps and considerably longer than that of the Nernst lamp; and entire freedom from all the complications of the Nernst lamp, the construction being the same in all outward details as the carbon filament lamp, for which it could be substituted in the lamp holder without trouble.

The tantalum lamp was not destined to remain long in the field unrivalled. The patent records for the years 1904–6 show that many inventors were busily engaged in improving the osmium lamp by using other metals and other methods for the preparation of the filament. Before long tungsten emerged from these experiments as the most suitable metal and two or three satisfactory methods of manufacturing tungsten filaments were worked out. Once again the Welsbach Company in Germany were to the front; the Osram lamp (a name compounded of osmium and wolfram) it produced being the first and still the most successful tungsten lamp.

The tungsten lamp does not differ in general characteristics from tantalum or carbon filament lamps. The filament is enclosed in an evacuated bulb and mounted on a wire frame in somewhat the same way as the tantalum filament: the tungsten filament, however, is not flexible and instead of winding a single thread over the frame, separate horseshoe filaments are mounted upon it and connected in series by the wires of the frame. The efficiency of the tungsten lamp approaches twice that of the tantalum lamp, the consumption averaging 1·2 to 1·4 watts per candle over a life of at least 1,500 hours. The life of the tungsten lamp can hardly be said to have been determined: 1,500 hours is probably a conservative estimate, lamps lasting 2,000, 3,000 and even as long as 5,000 hours without serious deterioration. Considerable difficulty was experienced at first in manufacturing lamps for high voltages (200 volts and over) but these difficulties are now overcome and lamps of the most common candle-powers are made for all voltages. The tungsten lamp is also made in very high candle power sizes, 500 to 1,000 candle power; in this form it is coming steadily into use for street lighting and the lighting of large interiors, a field hitherto occupied by the arc lamp alone. We shall refer to this matter later on.

We thus see that during the last ten years the invention first of the Nernst lamp, then of the tantalum lamp, finally of the tungsten lamp, has completely revolutionised the position of interior electric lighting. The remarkable progress is perhaps more clearly seen if the relative merits of the different types of lamp are set out in tabular form:

COST OF LIGHTING PER 1,000 CANDLE HOURS

Type of Lamp.	Consumption in Watts per Candle.	Approximate Life in Hours.	Price of Board of Trade Uuit.			
			1*d.*	2*d.*	4*d.*	6*d.*
			d.	*d.*	*d.*	*d.*
Carbon filament . .	4·0	800–1,000	5·0	9·0	17·0	25·0
Nernst . . .	2·2	500–600	2·6	4·9	9·4	13·9
Tantalum . . .	1·9	900–1,000	3·7	5·6	9·4	13·2
Tungsten . . .	1·2	1,500–2,000	2·4	3·7	6·3	8·9

A word may be said in reference to the question of life. There are three ways of regarding the life of an incandescent electric lamp, the simplest being to consider its life as the number of hours it runs before the filament actually fails. All the different types of lamp, however, deteriorate in use, the candle power steadily falling and the consumption in watts at best remaining steady and often rising. For a given lamp there is therefore a point, generally known as the "smashing point," at which it is most economical to discard the lamp and substitute a new one; this is the economical life. Unfortunately the economical life is very indefinite, depending on the price of the lamp, the cost of energy and the rate at which the watts per candle increase during use; moreover, as there is no obvious indication when a lamp has reached the smashing point, the economical life is not of any practical value. The third method, in many ways the most rational, is to say the lamp has reached the limit of its life when the candle power has fallen to a certain percentage of its original value: this is known as the "useful life" and 80 per cent. of the initial value is taken as the limit. This principle of determining life is based on the assumption that when the candle power is only 80 per cent. of the initial value it is no longer sufficient for the purpose for which the lamp was intended; it has the advantage that it is a principle which the user actually recognises, in a rough and ready way,

as most people change their lamps when they begin to give too poor a light. The useful life thus determined is generally considerably longer, often as much as double, the economical life but fortunately the loss in economy is not very great if the lamp is kept running until the end of its useful life: the actual life on the other hand is often very much longer than the useful life and it never pays to run lamps long after their candle power is noticeably small. In the table given above the figures refer to useful life.

Sufficient time has not yet elapsed for the general effect of the introduction of these high-efficiency lamps on the electrical industry to be apparent. At first, naturally, the central stations suffered; the new lamps were substituted for old ones in installations already in existence and there was a fall in the consumption of energy and consequently of revenue. But the process of recovery is now begun, as the new lamps make the competition of electric lighting with incandescent gas lighting possible and a steady increase in its use is to be expected confidently. It will doubtless be some time before the cheapness of electric lighting with tungsten lamps is generally realised but this once attained the progress should be rapid. As with all inventions which cheapen a product to the consumer, the ultimate effect on the industry cannot be other than beneficial; the cheapening in this instance being very marked—to about one-third of the old cost—the benefit should be correspondingly great.

Parallel with the development of the incandescent electric lamp, the small unit of light suitable for general interior lighting, there has been a no less noteworthy development in the arc lamp, the large unit suitable for exterior lighting and the lighting of large interiors. The arc is nearly a century old and arc lamps have been a commercial means of artificial lighting during a large proportion of that time but until about five years ago only the ordinary open or enclosed type of lamp was available. The open arc is an arc burning between two carbon rods in the free air, which is enclosed within a globe only to protect it from wind and weather. The arc itself is very short (about 2 mm.) and emits only an inappreciable percentage of the light given by the lamp, this being mainly derived from the white-hot ends of the carbons. In a direct current lamp, the positive carbon burns to a blunt point which is slightly hollowed out, forming

a small crater which is raised to an intense white heat: the negative carbon burns to a sharp point and only a very small area on this point is at a white heat; the bulk of the light therefore comes from the positive carbon. In an alternating current lamp both carbons are cratered and the light is emitted practically equally from each. The enclosed arc lamp differs from the open arc in that the carbons are enclosed in a fairly air-tight globe which, as they burn, becomes filled with carbon dioxide: in this atmosphere the carbons burn away much more slowly and the ends of both carbons are flat instead of pointed. A pair of carbons burning in such an enclosure will last ten or twelve times as long as similar carbons burning in the open air.

The improvement in arc lamps until the production of the flame lamp was but slight. As more experience was gained in the manufacture of carbons their quality was steadily improved, the ordinary commercial carbon of to-day being a very high-grade article both as regards purity and homogeneity, the two principal desiderata. At the same time there was naturally a steady improvement in the mechanism of the lamp by which the feeding of the carbons is controlled in order to maintain a constant length of arc and the length of arc regulated to correct slight fluctuations in the pressure of the supply. The only radical improvement was the introduction, fairly early in the history of arc lighting, in place of a homogeneous solid rod, of the cored carbon, in which a central canal in the carbon is filled with a special coring material consisting of finely ground carbon and potassium silicate. Being far more volatile than carbon, the potassium silicate vaporises into the arc, lowering its resistance and producing a marked steadying effect. The general practice for open arcs is to use a cored positive with a solid negative carbon for direct current and two cored carbons for alternating current: for direct current enclosed lamps two solid carbons are used and for alternating current one cored and one solid.

Compared with that of the carbon filament lamp, the efficiency of the arc lamp is high. The consumption in watts per candle is about 1·4 for open type arcs using standard quality carbons but by using the highest grade carbons containing a large percentage of lamp-black the consumption is reduced to about 1·1 watts per candle. The enclosed arc is less efficient, the consumption being about 2·3 watts per candle. The enclosed

lamp, however, on account of its longer burning hours affords some saving in the cost of carbons and a considerable saving in the expense of trimming—the recarboning of the lamps—which has only to be done once every 90–100 hours instead of once every 15–20 hours. The question as to which lamp is the better is therefore largely a matter of locality: where the lamps are scattered and labour is dear the economy in upkeep in the case of the enclosed lamp more than balances the extra cost of energy due to its comparatively low efficiency. So powerfully have these factors, especially the latter, operated in America that the open lamp was almost unused and the introduction of the flame lamp was delayed for a long time.

Various attempts were made from time to time to improve the efficiency of the arc by adding to the carbons compounds which would volatilise into the arc and cause it to emit light in addition to the light obtained from the carbons but no success was attained till in 1898–1900 Bremer succeeded in making satisfactory flame carbons by using calcium and other fluorides as an addition to the carbon. The original Bremer carbon was a solid rod composed of a mixture of carbon, calcium fluoride and other salts. Later practice has developed the flame carbon as a cored carbon having a very large core compared with the ordinary carbon: the actual diameter of the core is from one-third to half that of the carbon. The outer shell is pure carbon; the colouring matters are added only to the mixture used for coring. The composition of the colouring mixture has been simplified and the core now consists simply of carbon, potassium silicate and a fluoride: for the golden-yellow flame arc, the one in by far the most general use, calcium fluoride is used; a white flame is given by cerium fluoride and a red flame by strontium fluoride. The fluoride volatilises into the arc, converting it into an intensely luminous flame and greatly lowering its resistance. As a consequence of the lowered resistance it is possible to work with a very much longer arc, an arc in fact about ten times as long as the ordinary open arc. The flame is therefore large and contributes a large percentage of the total light obtained from the lamp. It is however possible to obtain a second advantage from the long arc. In the open type arc the shortness of the arc necessitates the use of two coaxial carbons, one vertically above the other; the lower carbon obstructs about 30 to 40 per cent. of the light emitted

by the crater. The long flame arc allows the carbons to be arranged side by side pointing downwards, so that the arc may burn across the lower ends. In this way the crater is fully exposed and the whole of the light emitted by it is rendered available.

The flame arc is more than twice as efficient as the open arc, the consumption being about 0·4 watt per candle. This is the figure for the yellow flame arc; the white and red are less efficient, consuming about 0·7 watt per candle. The determination of the cost of lighting with arc lamps is a more difficult matter than in the case of incandescent lamps. In the latter case it is only necessary to know the average consumption of watts per candle and the average useful life; the calculation of the cost per 1,000 candle hours is then a simple matter. With the arc lamp the prime factors in the cost of lighting are naturally also the consumption in watts per candle and the cost of carbons; but in addition the maintenance costs are considerable and unfortunately so variable from one installation to another that it is impossible to make any estimate that is of general application. Trimming costs will vary immensely according as the lamps are scattered over a wide area or grouped closely together: the extent to which the lamps are depreciated in value will also vary very greatly according as they are burning under cover or in the open. It is better therefore to give the cost taking into account only the cost of energy and carbons, which will be independent of the nature of the installation; on this basis the costs are as follows:

COST OF LIGHTING PER 1,000 CANDLE HOURS

Type of Lamp.	Consumption in Watts per Candle.	Price of Board of Trade Unit.			
		1*d.*	2*d.*	4*d.*	6*d.*
		d.	*d.*	*d.*	*d.*
Open arc	1·4	1·6	3·0	5·8	8·6
Open arc (high-grade carbons)	1·1	1·4	2·5	4·7	6·9
Enclosed arc	2·3	2·6	4·9	9·5	14·1
Yellow flame arc	0·4	0·6	1·0	1·8	2·6
White flame arc	0·7	0·9	1·6	3·0	4·4

If these figures are compared with those already given for incandescent electric lamps it will be seen that the tungsten

lamp can compete with either the open type or enclosed arc on the score of cost, because it must be remembered that to the figures given in the second table something has always to be added for maintenance costs whereas the figures given in the first table represent total cost. The fact that the tungsten lamp can now be made in 500–1,000 candle power units renders it all the more efficient as a competitor. There are few interiors so large that a 1,000 candle power lamp is not a sufficiently large unit for a satisfactory distribution of the lighting and there are numerous cases of outdoor lighting—as, for example, the lighting of side streets—in which this unit is amply large enough. The competition of the large tungsten lamp is likely therefore to grow steadily more effective, especially as confidence in the lamps increases and users cease to be afraid of the possibility of frequent heavy charges for renewals. It may be argued that the open and enclosed arcs are already necessarily doomed to extinction by the flame arc but it must be remembered that the flame arc is a far larger unit than any other, 1,000 candles being about the minimum and the candle power ranging up to 2,000 or 3,000 candles. Consequently it is not suitable for any but the largest interiors and is also too large for side street and similar lighting. Wherever a unit of 1,000–3,000 candles is not too large, however, the flame lamp must be expected to sweep everything else before it.

The question of the colour of the light is also an important consideration. The yellow flame arc is brilliant but not too pleasing. One advantage it possesses is a great fog-penetrating power but against this must be set the disadvantage that it produces a certain falsification of colour values. The white flame arc is neither so efficient nor quite so satisfactory in general working as the yellow, nor is the white absolutely a true white: about as good as the colour of the light from the tungsten lamp, it is not equal to that from the enclosed arc, which still remains the nearest approach to daylight of any artificial illuminant. Even the enclosed arc may be expected to continue in extended use especially where colour value is of special importance, as, for example, in drapery shops and so forth. It may be noticed in passing that the red flame arc is used almost exclusively in butchers' shops.

In all the figures which have been given the mean spherical

candle power has been taken as the basis of comparison. The mean spherical candle power is the candle power of a source of light radiating with equal intensity in all directions the same total amount of light as the source under consideration. No other candle power is of any value in a comparison such as has been made. It is important to remember this when figures for the candle power, efficiency and cost of lighting are considered. Figures double those which have been given for arc lamps will frequently be found: these represent the mean lower hemispherical candle power and are double those for the mean spherical, because most arc lamps give nearly all their light in the lower hemisphere. Their use is justified on the plea that it is only the light in the lower hemisphere that is useful: although this may justify the practice to a certain extent when the comparison is only between one type of arc lamp and another, the justification falls to the ground when they are compared with incandescent lamps or gas lamps. The mean spherical candle power is the only measure of the *total* light given by the lamp and therefore the only true basis for comparison.

No reference has been made to one or two electric lamps which have barely been advanced beyond the experimental stage, such as the magnetite arc, the mercury vapour lamp and the Moore vapour lamp. The magnetite arc uses electrodes of a mixture of magnetite, titanium oxide and chromium oxide forced by pressure into an iron containing tube. A white flame arc is produced with a consumption of about 0·7 watt per candle: as a compensation for the low efficiency compared with that of the flame arc the electrodes have a long life—from 100 to 150 hours. The lamp has been developed in America and is in use there to some considerable extent.

The mercury vapour lamp consists of an arc between electrodes of mercury enclosed in a tube of glass or quartz. The efficiency is high, the consumption being from 0·3 to 0·4 watt per candle but the light is almost entirely devoid of red rays and consequently of a colour that makes the lamp impracticable in all but a very limited number of cases. Some slight improvement has been effected by using quartz tubes which enable the arc to be run at a higher current density but there is still a long way to travel before a satisfactory result is obtained.

The Moore vapour lamp is a direct application of the vacuum tube discharge to electric lighting: long glass tubes are used, running for example entirely around the cornice of a room; these are filled with air or other gas at a very low pressure: a discharge at high voltage, 10,000 volts or over, is passed through this tube. The colour of the light, which depends on the particular gas with which the tube is filled, is good but the consumption is as high as 1·8 watts per candle and the cumbrous construction of the lamp hinders its general utility. In many ways an ideal form of lighting, notably in the admirable distribution of the light and the low intrinsic brilliancy of the source, the low efficiency and other drawbacks must prevent the Moore lamp competing with much success against either the tungsten or flame lamp. None of these lamps can yet be regarded as real competitors in the field of artificial lighting and until they have been improved sufficiently to qualify them in this way their more detailed consideration may be deferred.

From what has been written it will be seen that during the last five years a remarkable development of electric lamps has taken place which may confidently be expected to involve a no less remarkable development in electric lighting. It may even now be difficult for electric lighting to compete with gas lighting but this is due to the high cost of producing and distributing electric energy and not to the inefficiency of the means of converting it into light; though the result may be low absolutely it is relatively remarkably high. Even when the best that he could produce was represented by the carbon filament lamp and the open arc, the electrical engineer could boast that he had devised the most efficient light-producing apparatus to be found: the developments of the last few years appear to render this position unassailable. Nevertheless, there is ample room for improvement: the most efficient of the electric lamps consumes as much as 0·4 watt per candle. Could electric energy be converted directly into light without any waste as heat, the consumption would be only 0·08 watt per candle. Great, therefore, as the progress has been in the last five years, there is open to the investigator a field for greater progress still.

RADIOACTIVITY

By Professor W. H. BRAGG, F.R.S.

The facts and theory of radioactivity have been with us for several years and we have acquired more or less familiarity with the ideas which were at first so startling. In the case of those who themselves handle the radioactive substances and investigate their properties the first sense of strangeness quickly disappears, for they soon learn to have no doubt of the reality of the things with which they have to deal. To those, however, whose knowledge is indirect or who have had little time to follow a subject which has developed with sensational rapidity, the theory can still seem a provisional hypothesis and to such a brief restatement of the whole position may be of some use. Not that it is possible to give a complete historical and logical account in a short paper such as the present must be. The story is now too long. But perhaps something much less ambitious may be attempted which may serve a purpose. I propose therefore to state what seem to be the fundamental ideas of radioactivity, on the novelty and importance of which rest the claims of the new subject to be recognised as a science in its own right. And then, as it would not be possible to state all the arguments in their favour or to work out their consequences, I shall merely attempt their illustration by following up the investigations which have been made on the passage of the new rays through matter. Adhering to this limited inquiry it is possible, I think, to drive a line through the subject from various points on which may be seen the most of what there is to be seen; and I shall not be trying to do the impossible.

First, then, the fundamental ideas. These seem to me to be (*a*) the disintegration of the atom, (*b*) the existence of the electron, (*c*) the penetrability of matter to an extent not realised previously.

The first of these is due to Rutherford. Radioactivity itself is supposed by him to be due to a radical convulsion in the atom of the radioactive substance. The result is the expulsion

of a helium atom at an initial speed of many thousands of miles per second, the remainder of the atom forming an atom of a new substance, which is very generally found to be itself radioactive and to undergo the same process in its turn. Besides the projection of helium atoms generally termed the α radiation, there is also an emission of what are called β rays and γ rays; and it is by reference to the phenomena attending the passage of these various types of radiation through matter that I propose to illustrate the three ideas just stated. Rutherford's conception implies that one kind of matter is continually being derived from another, the latter disappearing to have its place taken by the former. Since different kinds of matter have different chemical and physical properties, it must therefore be possible by processes such as are already in common use to separate any newly formed product from the substance of its origin and to watch a fresh crop come into existence. Again, since the new product is in itself radioactive and may disintegrate at any moment after its formation, it can never accumulate to an unlimited extent. If, for example, a given quantity of radium be taken, the first product, which Rutherford called the emanation, goes on perceptibly growing in quantity for several days but at the end of a week or so a balance is reached. The number of births, so to speak, is practically a constant quantity all the time, for the number of radium atoms breaking down in a week is so small a fraction of the number of radium atoms present that no perceptible change takes place in the latter. But the number of deaths among the emanation atoms is growing, being equal to the product of two things, the one the number of emanation atoms in existence, the other their mortality. Each radioactive substance has its own mortality. We know nothing whatever of the reason for this; we do not indeed understand in the least why the break-up takes place at all. We can only say that it is a matter of chance. No sooner is a radioactive atom born than it stands the risk of dying; moreover, as experiment shows, the chance that it may die at any moment is quite independent of the time it has been in existence. There is no senile decay nor any process which tends to make a given atom at any time more likely to break down than any of its fellows, however it may differ from them in age. It was at one time held strongly that the process of disintegration was the outcome of a long-

continued decay of the internal energy of the atom, arising from the radiation spreading away through the surrounding ether. The radiation was ascribed to the action of the spinning electrons in the atomic system, for any motion involving acceleration can be shown to be the origin of radiation and therefore a cause of the emission of energy. But an atom which has come into existence as the result of a radioactive disintegration has no process to work through before its turn comes to pass away. The cataclysm may come at any time and needs no preparation to be made for it; and is all the more mysterious on this account. This is a convenient time to state also that the transformation is not to be precipitated or delayed by any of the agencies which physical and chemical science consider. Neither cold nor heat, nor even the most searching chemical action can affect its chance of occurrence. This alone places the new science by itself: it is more fundamental than any followed hitherto.

Now let us try to illustrate and verify some of these points by considering the properties of the α rays. We must, of course, suppose some way of detecting their presence and measuring their effect; and there are three such ways. One depends on the action of the rays on a photographic plate but we shall not use this. Another depends on the power of the α rays to excite phosphorescence in a screen of proper material; and we may observe by the way that in examining this effect carefully we shall find evidence of the material nature of the rays. For when the screen is examined under the microscope while the rays are acting on it, the illumination is seen to be due to many minute sparkles which appear on the screen, distributed irregularly in time and space and reminding us of the result of throwing a shower of pebbles into a phosphorescent sea. The third way depends on the production of ionisation by the rays as they traverse the air. There is no need whatever to examine or know anything about the exact nature of this process for the present. It is sufficient that the air, when traversed by the rays, is thrown into such a state that the application of an electromotive force across it causes a current to flow through it, which current does not depend, within wide limits, upon the magnitude of the electromotive force but does depend on the strength of the action exercised by the α rays. Thus the usual apparatus of battery, ionisation chamber and electro-

meter or electroscope, is no more, for the present at least, than an indicator of where the rays have been at work and a measurer of what they have done. It is this method which is used in the experiment I shall now describe.

In a report presented to the International Congress at Paris in 1900 an experiment is mentioned in which some radium had been placed close to an ionisation chamber with a wide opening through which the rays streamed in. The effect of the rays, as might be expected, varied with the distance of the radium from the chamber but there was a somewhat sudden alteration

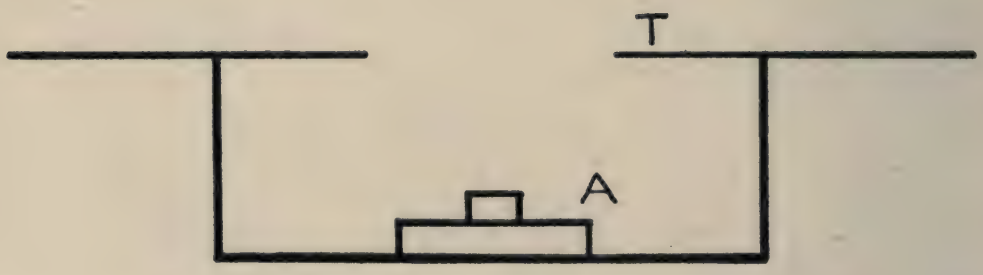


FIG. 1.

in the effect when the distance of the radium from the chamber was increased beyond a certain limit. As Mme. Curie says (see the figure which represents part of her apparatus), "For a certain value of the distance A T (at least 4 cm.) no current passes (*i.e.* there is no current in the chamber P P P′ P′ and therefore no ionisation there): the rays do not penetrate into the condenser (*i.e.* the chamber). When the distance A T is diminished the appearance of the rays in the condenser comes about suddenly so that a small diminution of the distance turns a very feeble current into a strong one: after that the current grows regularly as the radiant body [Mme. Curie was using polonium] approaches the window T."

This effect may be examined much more closely than in the original experiment. Consider an improved form of the apparatus as sketched in fig. 2. The chamber is far shallower than Mme. Curie's and for its lower plate has been substituted a sheet of gauze, through the meshes of which the *a* particle can stream if the radium be close enough. In order to simplify the interpretation of the experiment, let a set of little tubes be placed over the radium so that only those rays which are streaming almost vertically upwards can emerge from the top

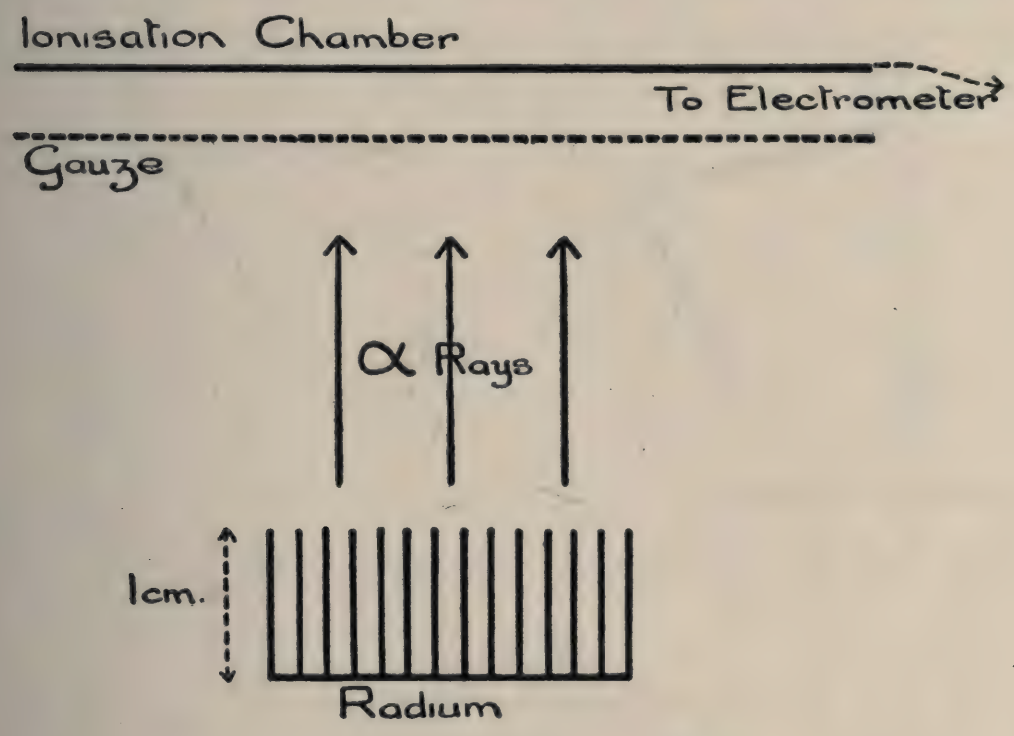


FIG. 2.

of the bundle. The radium is spread in a very thin layer over the surface of the plate; in fact, a drop or two of water containing radium in solution has been placed on the plate and evaporated down until the remainder seems merely a stain on the plate. The extreme thinness of the layer is also a factor making for simplicity, as will appear presently.

When the distance of the radium and its tubes from the ionisation chamber is varied as in Mme. Curie's experiment, the effect in the chamber is practically zero when the radium is more than 7 cm. from the gauze: a very slight degree of further approach causes a large increase in the current, which now grows rapidly until the distance is reduced to 6·5 cm. nearly.

Then it ceases to grow and indeed begins to diminish as the radium comes closer still: see fig. 3.

Let us now try to understand what this experiment must mean. In the first place a radiation which extends to a certain distance from the parent source and then ceases with extreme abruptness cannot be a wave motion. Wave radiation fades away gradually as it proceeds from its source and spreads over greater and greater surfaces; it never ceases abruptly while passing through a uniform medium. Only projectiles can have a range which can be measured with, it is found, an accuracy of

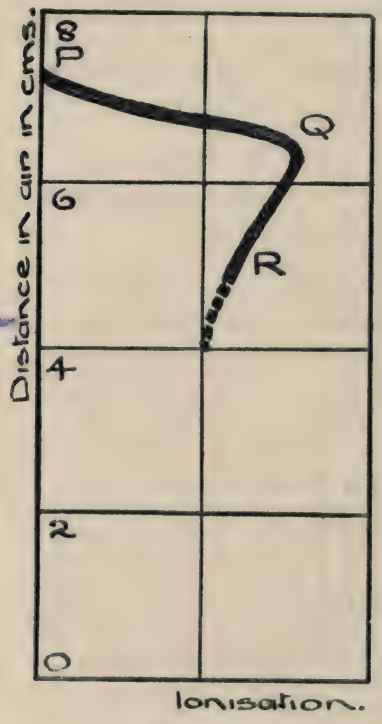


FIG. 3.

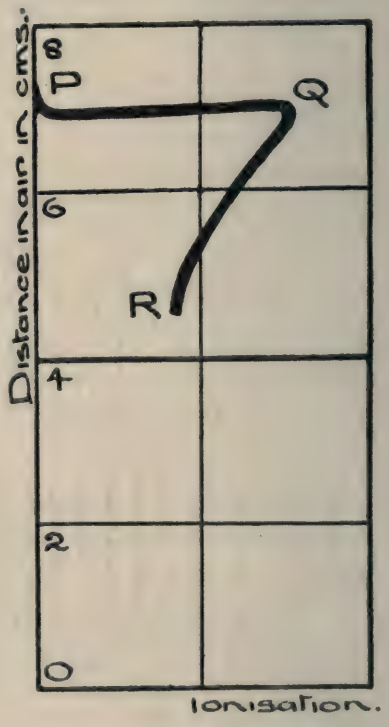


FIG. 4.

much less than 1 per cent. In the second place these projectiles must be slowing down as they come to the end of their range; they could not keep up the same velocity all the way and then all stop at the same moment. It cannot be a case of the accidental and chance stopping of the particles in full career. Since, therefore, the curve bends back from Q to R, the effect of the projectile measured by our apparatus is greater the slower it goes. The slope of the upper part of the curve P Q might be explained by supposing that the projectiles are fired off with slightly different speeds, the greater speed giving the longer range; but later experiments show this is not the case and that

fortunately the simple supposition may be made that they all have the same initial speed. Or again it may be supposed, as I originally did, that the radium layer has an appreciable thickness and that the particles from the bottom of it lose some of their speed on the way out. This turns out to be so far right that if the thickness of the layer be increased the slope from P to Q becomes longer; that is to say, there are particles which have come from so far down in the radium that they have not velocity enough to get so far as 6·5 cm.; indeed, if the layer be thick enough there are some that can only just emerge. But even when the layer is reduced to the extremest thinness there is still a slope at P Q; we can never get the curve to be quite as sharp as in fig. 4. Probably the explanation follows from results obtained by H. Geiger, who shows that when the particles are near the end of their course they are somewhat knocked about and thrown into paths which end at varying distances from the source. We will consider Geiger's work later.

Let us go on with the experiment. Suppose the radium is pushed somewhat closer, up to a distance of 4·8 cm., the effect of the rays in the chamber goes on steadily diminishing and then suddenly it increases again. The chamber has evidently come within the range of another set of projectiles and we find the same phenomena repeated. Again there is a rapid increase for an approach of about half a centimetre shown by the portion R S; there is a pause: and then (see fig. 5), just as we are apparently beginning to find the decrease corresponding to Q R, a third stream is struck and the phenomena are repeated a third time. This time the curve does get a little way round the bend, as at T U; then almost immediately a fourth set of particles is struck and for the last time we have a slope U V corresponding to the first slope P Q and a final return of the curve V W. Thus there appear to be four sets of α particles.

When this quadruple effect was first observed, its complete correlation with Rutherford's theory was obvious. Rutherford had shown, as already stated, that from radium proceed other substances in successive derivation, the which substances are also radioactive. The exact genealogy is as follows: Radium itself has a low mortality or, as we may say, a long average life; in fact, the average life is now supposed to be about twenty centuries. Its immediate descendant is the emanation which has a far shorter life, for its atoms only live some three and

a half days on the average. Radium A springs from the emanation and its average life is shorter still, some three or four minutes only. From A spring in turn radium B and radium C, each of these living about twenty minutes on the average. If therefore, as Rutherford showed, radium were purified of its products and left to itself, the descendants would be present in full force in a few days. In addition to these short-lived products there are succeeding generations; but the first of these has an average life of some forty years and since it is only manifest in its death, it and its successors give us an

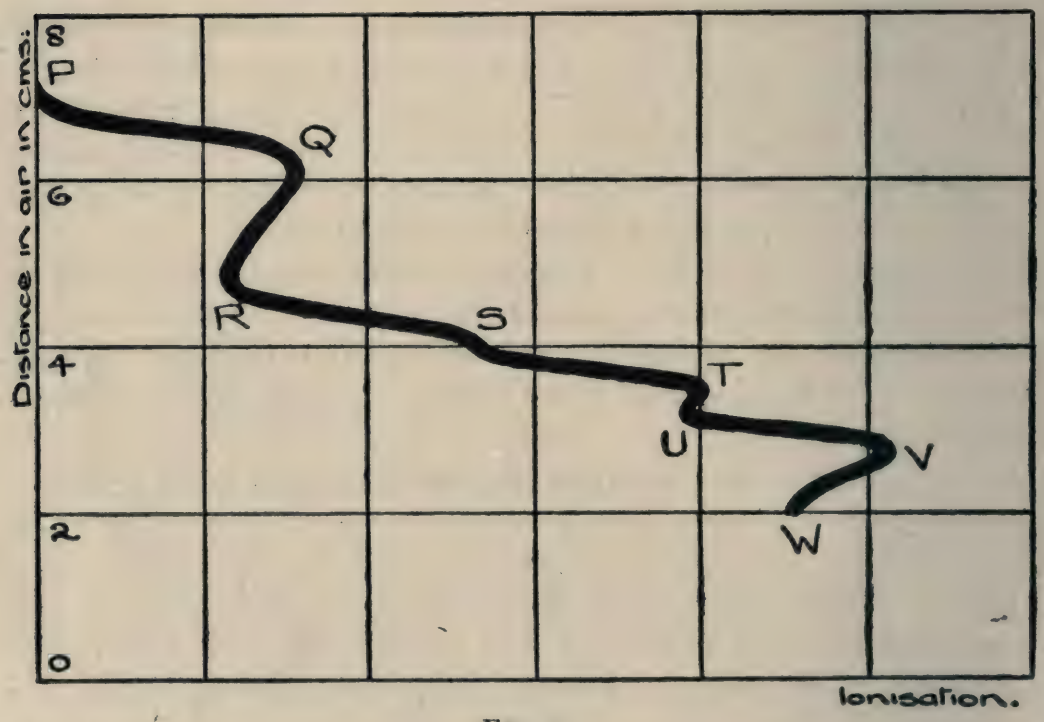


FIG. 5.

effect which is comparatively negligible. Lastly radium, the emanations, Ra A and Ra C all emit particles but Ra B does not. The bulk of these brilliant deductions of Rutherford's can be illustrated by our experiment. Clearly we can account at once for the four sets of corpuscular radiations; they must be four lots of α particles from the four substances which emit them and no doubt if we proceed further we shall be able to assign each lot to its own source. But in the meantime we can see a clear proof of a very important feature of Rutherford's theory. When the short-lived products are present in full, they exist in such quantity that the number of atoms of each coming

into existence in any second is equal of course to the number disintegrating in the same time and producing the next in order. Consequently there must be equal numbers in each of the four α particle streams. Now, if we examine by scale the curve in fig. 5 we find that it is compounded of four curves exactly the same in every respect but that of vertical height. This is just as it should be, if we make the further supposition that the α particles are all alike though they are the product of different radioactive substances.

If we could isolate for a sufficient time each one of the radioactive derivatives of radium and make a thin layer of it upon a plate to be then placed in our apparatus, we should no doubt be able to see its simple curve. We should learn which of the radioactive substances gives rise to each of the different sets of rays: we should know the "range" of the α ray from each substance and be able to use it as a distinguishing characteristic. It is just possible to do this but with difficulty; and we may consider other experiments which, though a little less easy of interpretation, are much easier to perform. Let us remind ourselves that the emanation, radium A, B and C are all different substances and can be separated by physical or chemical treatment.

For example, the emanation is easily driven off from the radium by the application of heat. Let us heat to redness the plate on which the radium rests, replace it in the range-finding apparatus and remeasure the ionisation effects. We now obtain the curve as represented in fig. 6. By comparison with fig. 5 we see that the two centre blocks have disappeared. But why two? We should have expected the effects of the emanation to have been removed but we see that a second has gone at the same time. There is of course a ready answer which does not require us to suppose R A or R C to be readily volatilised. It is that Ra A has a life of only three or four minutes. When therefore we drive off the emanation by heat and the origin of Ra A is removed, the store of Ra A is soon used up and we have not been quick enough in our experimental arrangements to catch the effects of the disappearing product. Indeed, unless we are fairly quick we shall not see much of the effect of the Ra C either, for its average life is under half an hour; and in fact the form of the curve alters as we measure it. In a very few hours it takes the form of fig. 7: the top block

having gone. Now we are in a position to say which α stream belongs to each substance. The block which is left behind must be due to radium; so the particles from radium have an extreme range of 3·5 cm. and are less energetic than those of any of its derivatives. The particles of longest range are those of Ra C; the two intermediate ranges belong to the emanation and to Ra A. Between these last two this experimental method cannot easily decide, though it might be made to do so. But H. W. Schmidt has shown by an independent method that the shorter range belongs to the emanation. Each of the successors

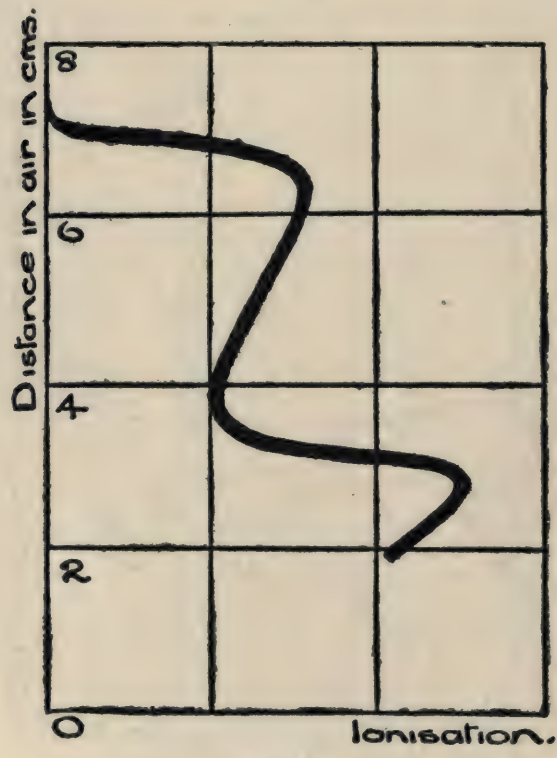


FIG. 6.

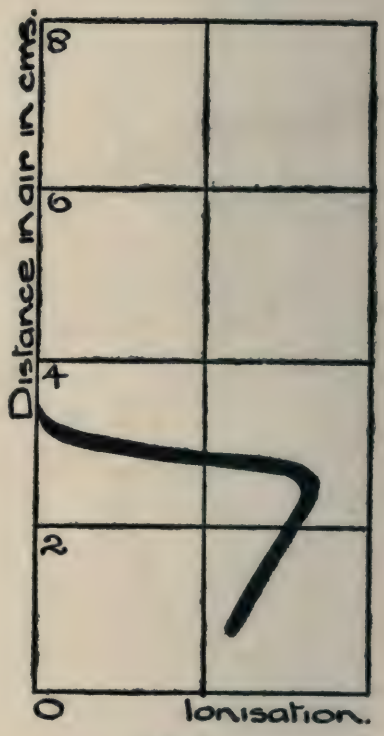


FIG. 7.

of radium up to Ra C therefore expels its α particle more violently than those that have gone before.

Now for a last and very interesting test of Rutherford's theory. The radium plate which has been cleared of its products will grow them again; therefore if we continue our experiments and examine the curve from time to time we ought to see it recovering gradually its old form. The time of growth is governed by the life of the emanation. Since it has an average existence of between three and four days, it will rise to half its final value in about that time: formed at a regular rate, it accumulates at a diminishing rate, since

the rate at which it disappears is proportional to the quantity in existence. The life of the others is so short that their growth cannot be observed in comparison with that of the emanation and they seem to follow closely at its heels. In fig. 8 is shown the form of the curve at different times after the purification of the radium; it will be seen how excellently this surmise is fulfilled. The radium block is unaltered in form as it moves slowly to the right: the others grow together day by day and after a week or two the curve shows that the normal condition of things has again been reached.

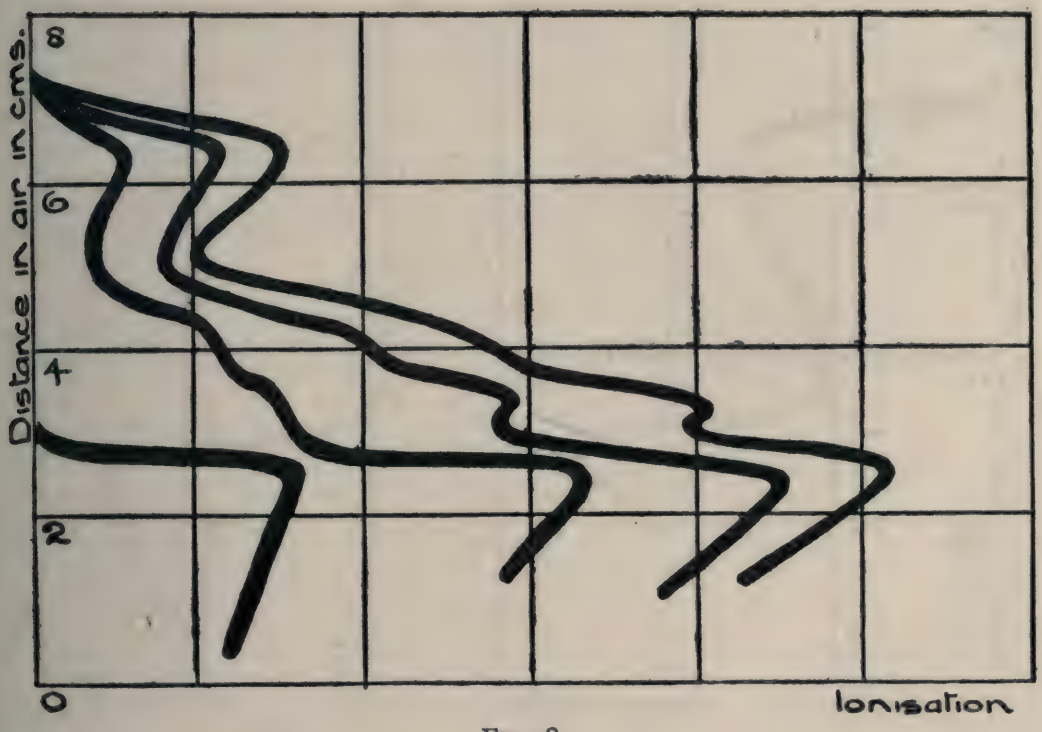


FIG. 8.

The four curves in the figure going from left to right were obtained (1) directly after preparation of the film, (2) after 64 hours, (3) after 140 hours, and (4) after 28 days respectively. See *Phil. Mag.*, Sept. 1905, p. 323.

All these experimental tests of Rutherford's theory, using the newly discovered properties of the α rays, were made after the main facts of the theory had been worked out by quite different methods. It is therefore the very best testimony that such a theory could have; in every detail the effects observed answered to such expectations as could be formed and where they revealed fresh facts or laws they dovetailed in with the old in a most satisfactory manner. Take for example the

proof that the *a* particles of one kind had all the same initial speed: though it must be admitted that the first evidence was somewhat over-valued and more satisfactory proof was not found until later. This showed that the cataclysm which resulted in the birth of the *a* particle was always the same and that each radium atom in turn underwent exactly the same experience, an idea which gave precision to conceptions previously formed.

There were certain new facts which appeared from the observation of the behaviour of the *a* rays which wholly or in

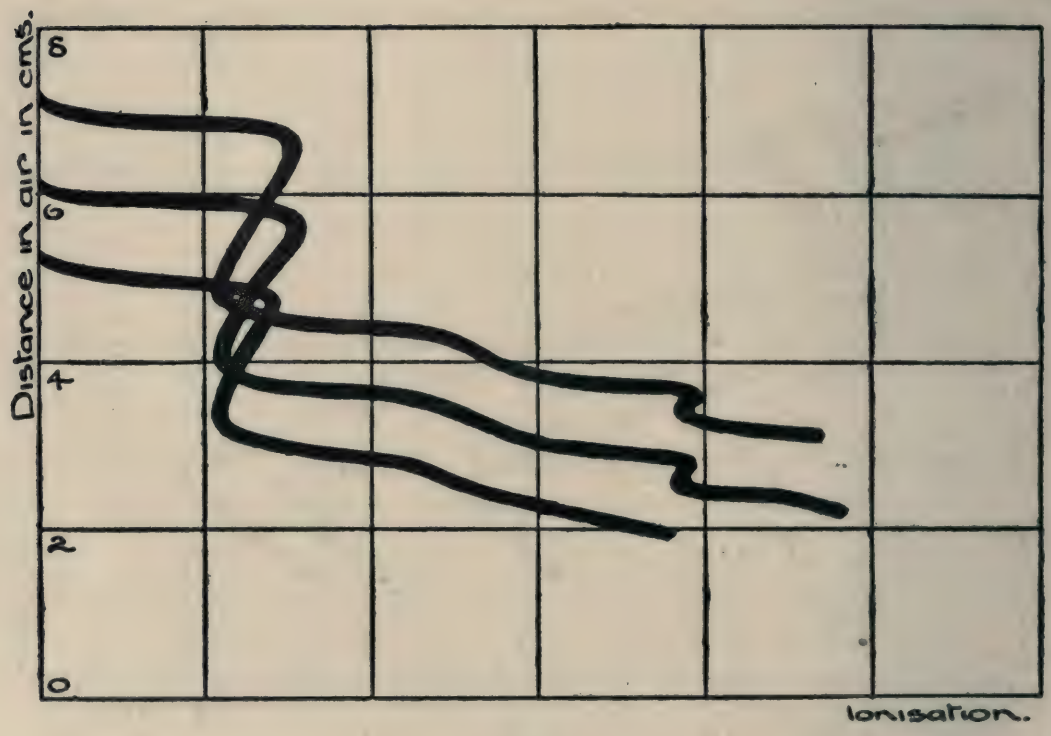


FIG. 9.

The three curves in the figure beginning with the top one were obtained with the radium film (1) uncovered, (2) covered with a silver sheet weighing ·0021 gr./cm.², and (3) covered with a platinum sheet weighing ·0063 gr./cm.². See *Phil. Mag.*, Sept. 1905, p. 331.

part had not been anticipated. Let us consider these for a moment.

It is natural to test radiations by considering their ability to penetrate sheets of different materials: indeed it is really the most interesting fact about all radiations. Let us place therefore a thin sheet of some substance in the way of the *a* rays of our experiment and examine the result. It is certainly most surprising. It is found that every *a* particle loses a

definite portion of its range: the whole curve is lowered by the same amount, as shown in fig. 9. The drop in different parts is not *quite* the same but for present purposes this variation is not worth consideration. Now this can only mean that *every α particle has gone through the screen without losing its original direction of motion.* If any had been lost in going through, the curve would have shrunk sideways as in fig. 10; the intensities of the various streams would have been diminished. There is no sign of this. If the particles had been much scattered the curve would have lost its feature and become

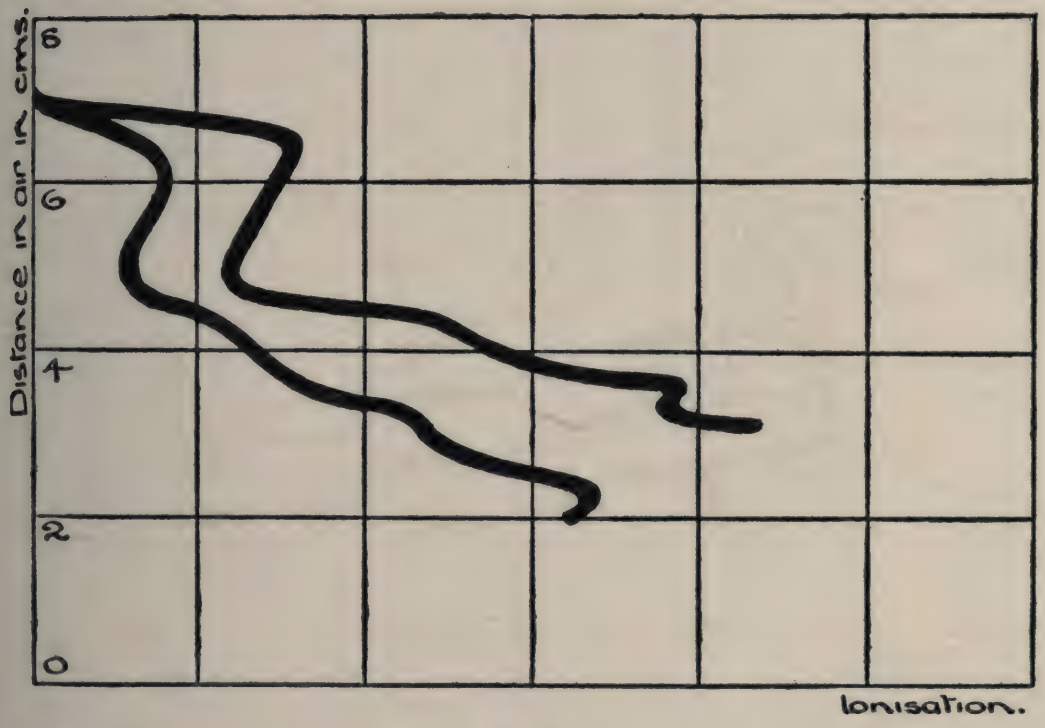


FIG. 10.

blurred and we might have got something of the form shown in fig. 11. But the curve loses no detail, as is shown in fig. 9; no conclusion seems possible other than that already stated.

Every *α* particle therefore passes through the screen with the same loss of energy and there is no appreciable deflection of the particle from its original line of motion. Now the particle is a projectile; and it must therefore have kept to the same line of motion all the way through the plate, for it has no intelligence and it could not recover a line once it had left it. It has therefore had no collisions to deflect it. But its line of motion through the plate must have taken

it over millions of molecules of the plate material. It could not have pushed them all to one side unless their combined mass was much less than that of the particle itself, which is certainly not the case. What other conclusion can we come to than that it must have gone *through* them all, since it cannot have gone round them?

Then we naturally ask, what then is this *α* particle and how does it come to possess this extraordinary power?

It is of course abundantly proved that the *α* particles are helium atoms. The continual presence of helium in materials containing radioactive matter would naturally point to this

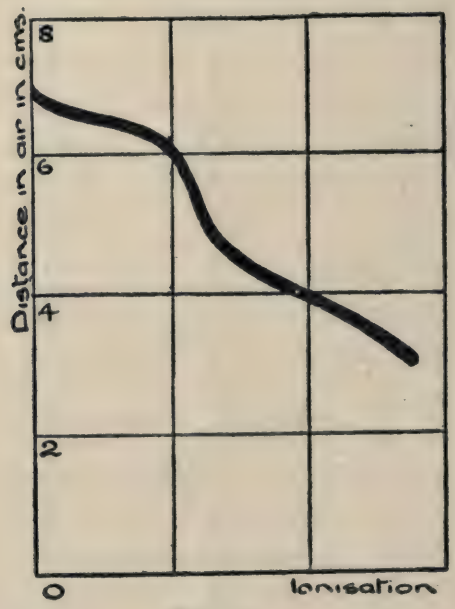


FIG. 11.

conclusion and various workers have actually grown helium from radium. But the most simple and direct proof is due to Rutherford. Placing some emanation in a glass tube with very thin walls, on the outside of which a vacuum at first existed, he found that helium gradually appeared in this outside space. Without the emanation nothing of the kind took place, so the radium emanation shot helium through the glass wall. When the emanation was withdrawn from the inner tube and helium was substituted for it, nothing of the sort took place. The effect is not due to a leak.

Our experiment on the passage of the *α* particles through the screen shows therefore that the helium atom must pass *through* all the millions of atoms it meets in the screen. Here

then we find a beautiful illustration of a prominent feature of the radioactive theory. The atom is to be considered as quite an empty thing. It has parts no doubt; we find it best to represent them simply as centres of electric force; but they are perhaps few and certainly far between, so that one atom sweeps easily through another, with perhaps never a clash. But why is it only now that we find this effect, why have we never discovered it before?

Again let us go back to Rutherford's experiment. By passing the α particles through electric and through magnetic fields he found they were deflected in both cases; and making the usual calculations he found that the velocity was enormous; *e.g.* the initial velocity of the α particle from Ra C was some 20,000 miles a second. At the same time it appeared that the charge the helium atom must be carrying was twice the charge carried by the hydrogen atom in electrolysis.

Here is a novel enough condition. At the hottest temperatures we meet with, the atoms of a gas never move with a speed approaching to this in the slightest degree; at ordinary temperatures the molecule of hydrogen has a speed of only 186,000 cm. a second. This is surely enough to explain matters; and we ascribe the motion of the molecules through one another to their terrific speed. Physics and chemistry concern themselves with the interplay of atoms which meet surface to surface only: each atom has its own domain which it keeps to itself and has relations with others at its boundaries only. But now we have an entirely different state of things, to which surface actions between atom and atom are of no significance at all. The atoms can pierce each other if they approach with sufficient relative speed and we are very ready to believe that under conditions so new to us we must find effects that are unfamiliar.

Then does nothing happen to two atoms which pass through each other in this way? There is at least one consequence—viz. that which has been termed ionisation. Some of the atoms through which the α particles pass are found to be charged, though they were neutral before; the helium atom itself is always charged. It is to be remembered that the latter has many encounters, including the first encounter or shock of its separation from the parent atom. The helium atom is positively charged and we may infer that the atoms

it traverses become positively charged also, though this is by no means the only evidence of the particular sign of the charge. I will return to this question of ionisation later on and will only say now that not only is the ionisation the result of the interpenetration of the atoms but in all probability the ionisation of radioactivity has no other cause; when it occurs it is a sign that the "domain" of the atom has been broken into by some other atom or at least by an electron.

And there is one other important result of the mutual crossing. The α particle loses in energy in the act and therefore in range, as we have already seen. It is really not quite clear whether it loses energy only where ionisation takes place or whether it always loses energy and ionisation only sometimes follows. Certainly the same expenditure of energy in different gases does not always produce the same amount of ionisation. The significance of this will only appear in full when we have gone a little further with the experiment we are imagining to be in progress; I will call attention to it at the proper time.

Let us place screens of different metals in the path of the α rays or allow them to penetrate as far as they will in different gases: and let us compare the losses of energy (or of range) which the α particle suffers in going through equal numbers of atoms or molecules. A very remarkable law appears. The loss of range in going through any atom is very nearly proportional to the square root of the atomic weight. Thus an atom of silver takes away twice as much energy from the particle as an atom of aluminium, being four times as heavy. There is no very obvious explanation of this result to be offered at present. It is so striking and peculiar that it is sure to have some very important meaning if we could but get at it. One other law appears, also of great simplicity and much more easy to make something of. Suppose the substance penetrated by the α particle to consist of compound molecules, solid or gas: we then find it possible to anticipate with exactness the loss of range which the α particle will experience in passing through it. We have only to allow for the separate atoms, without considering any molecular combinations they may have. The loss of range due to a molecule of methyl bromide (CH_3Br) is proportional to $\sqrt{12} + 3\sqrt{1} + \sqrt{81}$: that due to benzene ($C_6H_6$) to $6\sqrt{12} + 6\sqrt{1}$. Linkings and groupings and "rings" are of no

account. The fact is that when an α particle goes through a molecule it goes through one or more atoms in that molecule and its concern is with the inside of each atom it goes through: and it has never to do with more than one atom at a time. This gives a peculiar feature to all the radioactive phenomena, since every other of the radiations shows the same effect. The β, γ and X rays, as well as the α rays, pay no attention to chemical or physical conditions.

There is one other α particle effect which we may well consider for a moment. It is well known that a particle when it strikes a phosphorescent screen causes a minute flash at the point of impact. Rutherford and Geiger have greatly developed this method of following the movements of the particles and the latter has shown by means of it that there actually is a small amount of scattering of the α particles on their way through matter. The experiment we have considered above does not show it obviously: and that is not surprising, for Geiger shows that when a stream of particles strikes a plate perpendicularly to its surface, only about one in eight thousand returns. When, however, the velocity falls off very much—it drops to almost nothing at the end of the range—scattering is more pronounced; and the α particles deviate considerably from the rectilinear path. This is probably the reason of the slope PQ in fig. 3. Being much knocked about at the ends of their paths, some of the particles will not go quite so far as others and the ionisation due to the particles does not cease abruptly as it would do if all the paths were quite straight and therefore of the same length.

Let us now pass on to the consideration of some of the β rays phenomena. In these we find illustrations of the second of the fundamental principles I have stated above in the existence and the separate existence of the electron. For these rays are actually streams of electrons the same as those which Thomson was the first to examine quantitatively. There can be no doubt as to the identity, for we can proceed by almost complete gradations from the slowest to the fastest moving of the kind and find no break. Thomson worked with the electrons of the cathode rays in the vacuum tube. As is well known, he measured by many methods the charge and mass of the electron and firmly established its existence and its uniformity, no matter from what source it is derived. The β ray when examined by

his method turns out to be merely an electron moving faster than any cathode ray. Taking it in with the others already investigated, we have a complete series of electrons moving with all speeds ranging from a certain important limit, which will be discussed presently, up to something just less than the velocity of light. There could be no point in a discussion here of the beautiful experiments which go to the determination of the electronic charge and mass: they are to be found in full and logical statement in the original papers and in many resumés. My purpose is rather to consider the significance which the fast-moving electron has in relation to the new science. I shall therefore assume that the electron of the cathode ray or the β ray, the electron set free by X rays, γ rays, ultraviolet light or any other agency, is always the same thing, having no variable characteristic but that of speed; and I shall limit these considerations to the phenomena attending the passage of these rays through matter and especially to the influence on them of the speed of the electron. Even by confining the discussion to these limits, it is not possible to consider the β ray as systematically as the α ray, for the experiments are more complicated and more difficult to interpret. The β ray has always been a far harder problem than the α ray. Even now every one is not of the same mind as to the mechanism of the absorption; but certain main principles seem to stand out with some clearness and I shall not blur their outlines by too much qualification but prefer to run a little risk of inaccuracy as to details.

The very difficulty and complexity of the β ray problem is due to a most instructive characteristic of the ray—viz. its extreme liability to deflection as compared with the α ray. The latter, by virtue of its comparatively large mass, moves undeflected through places where the β ray is easily swung to one side. The α particle, according to Rutherford, has twice the charge of the β particle numerically (the signs are opposed but that does not matter) and is several thousand times as massive, so that the difference in their behaviour is easily explained. A stream of β rays therefore very quickly loses its sharpness of definition and becomes diffuse and irregular: moreover, every single particle, though it sometimes possesses a higher speed than the α particle, has far less energy and we must use large numbers to get a measurable effect. Taking these points into consideration, it is easily seen that the experimental difficulties

are much greater for β than for α rays and it is not surprising that we have been longer in arriving at the same understanding.

Considering a single β particle there does not seem to be much difficulty in picturing its motion. It is a projectile, fired off with a certain initial speed and left to take care of itself. It goes right through atoms as the α particle does but it is continually being swung to this side and that by encounters with centres of electrical force which the atoms contain. The extent to which this takes place depends on the speed to a most surprising extent. The β rays from radium will keep a line fairly well in passing through several centimetres of air and many will be found continuing the old line of flight at a metre away from the source. The speed of these rays is just short of that of light, say $2{\cdot}9 \times 10^{10}$ cm. per second. Cathode rays which have one-third the speed can only reach a distance of two or three millimetres in air from their source. So we can go on, down to rays which have a speed of 10^8 cm. per second or thereabouts (600 miles a second) and these cannot penetrate a single molecule; they have no power of penetration at all. Neither can they ionise the molecule: it seems as if penetration must precede ionisation. Thus the ease with which β rays can be handled in comparison to cathode rays is due simply to the triple increase in speed; they can be made to travel far longer paths and to pass through thicker screens.

The path of the β ray is therefore very irregular and broken. Let us imagine it begun. It goes for a little time through the atoms; it meets with little or no deflection: it has not yet gone close enough to one of the strong centres of force within. But sooner or later the inevitable happens and there is a change in the direction of the movement, more or less severe. Now, does the β particle lose in speed at such an encounter? The right answer is not quite clear; but I think the negative is the safest, for the reason that when β particles are thrown against a metal screen large numbers of them are returned and the speed of the returned or "secondary" rays, as they have been called, is not much less than that of the incident. There is a little difference but it may be due to a kind of sorting-out process, the slower β rays in a bundle being more readily returned. At any rate there is not in general any great loss of speed; it is to be remembered that if we find a 10 per cent. change of penetrating power on reflection, which is much more

than is the case generally, this means a change in velocity on 1 or 2 per cent. only. Next we may ask ourselves—Did the deflection have any action on the atom in which it took place, such as ionisation? It is really very hard to answer this question: it may be that ionisation occurs in cases where the atom is simply traversed without a deflection ensuing; and, on the other hand, that deflection occurs without ionisation. Again, it may be that ionisation occurs *only* when the action and reaction exceed a certain limit; when this is passed the ionisation of the atom occurs and the β particle is deflected through a certain angle at least. We do not really know whether there is any connection between ionisation and deflection.

So the β particle experiences one deflection after another and whether it loses energy only at the moment of encounter with the centres of force within the atom or whether it loses energy during rectilinear portions of its path as the α particle does, it certainly loses energy somehow; for several experiments have lately made this clear. Does it then gradually slow down or does it ever become incorporated within an atom while still going at a high speed? It is so hard to trace the slowing down completely that many workers have supposed that the ray often disappears within an atom while still going fast and never emerges again. For my part I am inclined to think that this last effect does not occur and that an atom has no power to pull up and retain a fast-moving electron: it may deflect it or it may rob it of some fraction of its energy, perhaps only a small fraction; and that is all it can do. On the other hand, the slower an electron goes, the more rapidly does it spend energy on ionisation, just as the α particle does; moreover, there can be no doubt that the slower it goes the more likely it is to be deflected. Once it starts on the downhill path, its end is very precipitate; and it might seem as if it suddenly disappeared. In reality its track would be a variant of something like that shown in fig. 12. If this be the case, every β ray is a cathode ray before it is done; the distinction between the two is only a matter of speed; slowing down still more, it reaches the speed at which it cannot emerge from the atom once it has gone inside; this is the critical speed of about 10^8 cm. per second or that which a few volts of electromotive force, say ten, can generate in an electron. This last point has been made clear by experiment; it has been shown that unless the speed exceeds this limit no

electrons rebound from a plate on which they fall and that within certain limits the percentage returned increases with the speed.

It is interesting to observe that the treatment of β particles by atoms of various kinds through which they pass does not appear to be quite the same. Heavy atoms cause far more deflections in comparison with the amount of energy abstracted. Perhaps the centres of force within them are more powerful or more concentated. Rutherford has lately put forward this view in consequence of certain experiments on the scattering of the α particles. Thus the track of the β particle through lead is more broken than in aluminium and stretches to a smaller distance from its origin. If we might take fig. 12 to represent the track in aluminium, then in comparison fig. 13 would represent a track in lead; and this is why a lead screen is so much more effective in stopping β particles than an aluminium screen of equal weight.

There is one very interesting point in connection with the

FIG. 12.

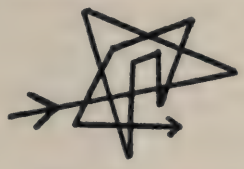


FIG. 13.

β ray which we may consider now. It may be put as a question: Does the electron as it moves through an atom prompt the emission of another electron by or from that atom with a speed in considerable excess of the critical speed, so that there are two rays where there was but one before? And in particular, if it does, whence comes the energy of the new ray?

If the atom can be stimulated to the expenditure of its own energy on the emission of a new β particle, we have a true secondary radiation. It was very commonly held at one time that such an effect existed but the trend of experiment has been very much against it; no one seems to maintain this now. The β rays which emerge from a substance on which a primary stream of β rays falls were at one time supposed to be a radiation of this sort. But they are found to resemble the primary rays closely, being in general just a little slower; the nature of the reflecting material seems to be of little influence except in indirect ways which would naturally be expected. There is nothing to show that the atoms of the

material have been stimulated to the exertion of some characteristic effort. It is a matter of very great importance because it makes all the difference in the world to our argument whether we have to take into or may leave out of consideration the possibility of any stores of energy belonging to the atom contributing to one side or other of our energy equations. It seems to me to be safe to deny the effect in this case because we should surely find a real induced radioactivity, such as this would be, to depend in quality on the nature of the atom, not on the nature of the agent that merely pulls that trigger. The very term

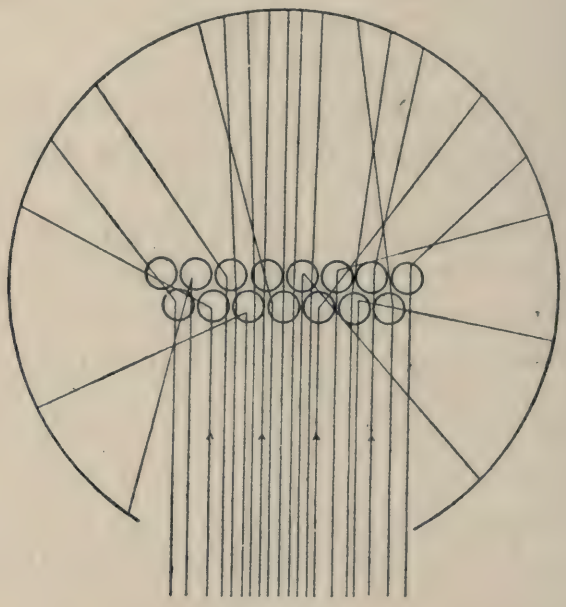


FIG. 14.

This figure is intended to give an idea of the way in which β particles are deflected in going through the atoms of the screen. A particle may go through numbers of atoms with hardly any result; and then in crossing some one atom it may go so close to a powerful centre of force within it that there is a violent deflection. The circles stand for individual atoms.

"induced radioactivity" was only introduced in the early days of the new science under a false impression. It was thought possible to make substances temporarily radioactive by exposing them to the radium emanation; it was soon discovered that in reality a radioactive material was deposited on the exposed substance and the latter was as idle as before. An induced radioactivity must at least be characteristic of the substance in which it is induced.

There is another reason against the existence of the effect. If atoms were prompted to activity in this way we should expect the new β rays they emitted to issue in all directions round

the atom; whereas observation shows that the emission is concentrated largely round the original line of movement of the stream of β particles which it more or less continues. Most of those which move off the original line are only slightly deflected and a much smaller number are turned through a large angle. Clearly this is characteristic rather of the simpler hypothesis we have considered first: it is just what we should expect if the original β particles were scattered or deflected and there is no need to suppose a new emission. In order to see this effect we must use a thin plate, say a tenth of a millimetre of aluminium, allowing the β rays to fall perpendicularly upon it. In that case the β particles have usually suffered only one large deflection, if any; we see the effects of single collisions, as shown in fig. 14. But even if we put aside the hypothesis that the internal energy of the atom enters into the question, there still remains the possibility of a sort of secondary β radiation arising in another way. A β particle might strike another electron in the atom which is crossing, with a blow so direct that the latter electron is driven out with great speed; so great that we may be said to have two β rays where we had one before. It would be a very great advance on our knowledge of atomic and subatomic mechanics could we give a definite decision on this point. If it could happen, then it would be still easier to understand the seeming disappearance of the β particle while going at a high speed, for with its full energy it often crosses a metre of air and it has but to divide its energy equally with two other electrons in succession and it can only cross three or four centimetres and so on. But there is no clear evidence that such a sharing ever takes place. When an α particle goes through an atom it or some part of it must sometimes hit an electron as squarely as a β particle could do. Yet we find no β particles springing out from the atoms traversed by the α particle: it is true, of course, that they might exist in too small numbers to be observed. We do indeed find ionisation and we suppose this to mean that electrons are ejected from the atoms which the ionising particles, α or β rays, traverse. But these electrons have little more than the "critical" speed already mentioned. In the case of the α particle there has been some measurement of their speed and it is clear that it never exceeds this critical value by more than a small margin, no matter what the ionising agent may be. This process, the real process of

ionisation which is at the root of all our measurements, is one of the hardest things of all to understand. We can do little more than put together the main items of our knowledge in respect of this: much more experiment is required before we can picture it to ourselves completely.

We know in the first place that the ejected electrons, δ rays as J. J. Thomson has called them, have a speed above the critical speed. This is a matter of course; below that speed an electron cannot break away from an atom. And, as already said, in no case does the speed seem to rise much above it, though this is only clearly established in the case of ionisation by α rays. It results from the penetration of the atom which is so great a feature of radioactivity. An electron can itself ionise as soon as it can penetrate. We do not know where the energy comes from with certainty; it is clear that an α particle for example loses energy while it is ionising but it is not clear whether it spends the energy in giving impetus to the so-called δ rays. The reason for this statement is that the amount of energy spent by the α particle in crossing any atom is in no way dependent on any association of that atom with others; clearly the energy is spent inside the atom, that is to say in that part of the atom which is not affected by chemical bonds of union with other atoms. But the amount of δ radiation emitted by a molecule cannot be predicted from a knowledge of the number and the sort of atoms that go to make the molecule; chemical constitution influences the ionisation. Moreover the amount is generally larger for complex molecules than simple ones, whereas the first effect of complexity that would naturally be looked for would be a diminution on account of the atoms of the molecule hindering the emission of electrons from each other. In the case of pentane (C_5H_{12}) for example, the ionisation is some 35 per cent. greater than in air for the same expenditure of energy and there is no obvious reason why it should be. The energy spent in the molecule on the other hand is found to be exactly what could be predicted on the additive square root law, proportional that is to say to $5\sqrt{12} + 12\sqrt{1}$. The binding together of the atoms has in some way made ionisation easier but it has had no effect on the motion of the α particle. The most natural interpretation would seem to be that the electrons separated as δ rays come from the surface of the atoms, from those portions of it which are directly concerned in the linkage of the atoms together.

There is some support to this idea in the fact that if we collect together the figures expressing the relative ionisations of various molecules by the α ray (or indeed by the β ray or cathode ray—the figures run very parallel in all these cases) they seem to show relations to known constants of the molecules such as the so-called "volumes." These latter do not of course possess the additive property.

Ionisation in radioactivity is therefore the result of intrusion into the atom; which, again, follows on excessive speed of approach. It does not require the help of an electric field; nor indeed would an electric field of any strength we could bring to bear be able to effect it by itself. We could quite well imagine the greatest electric field attempting to force a discharge through a gas and utterly unable to do it. But if there were one stray electron in the field this would move in the field and might in the ordinary length of a free path or less get up speed enough to penetrate an atom. Ionisation would result, with the addition of a new free electron; so that ionisation would multiply itself and a discharge set in. But we know of no case where the penetration of the atoms of a gas by α or β rays does not result in the emission of δ rays and so of ionisation which can be gathered up by an electric field.

The possibility of extreme penetration of matter is still more strikingly illustrated by the properties of the γ and X rays, though indeed the force of this statement will depend somewhat on what we consider these rays to be. But it is remarkable enough to be handling X rays which can penetrate inches of wood and γ rays which can pass through inches of steel. It is to be remembered that this is no diffusion process nor is it like the filtering of light through a fog: the rays go straight through—some of them at any rate—and proceed on lines radiating from their origin, so that they can cast shadows of objects which they afterwards meet. They have not turned aside to avoid atoms or parts of atoms but have gone through everything.

The γ rays are to be traced, like α and β rays, by the ionisation which they leave in their track; and like the other rays, they can also cause phosphorescence and they act on a photographic plate. They have, in addition, a very remarkable property; they excite β rays in any matter on which they fall. This leads us to ask whether the ionisation due to the γ rays

is direct or indirect; may it not be due to the β rays which they produce? For we know that the β rays ionise and we ought not to provide two causes for one result until we are sure that the one already demonstrated is insufficient. We can only answer this question directly by quantitative experiment. The description and discussion of such work would take too long; I can but state my own opinion of the results obtained. So far as it has gone, no reason is shown why we should suppose the γ rays to be able to ionise as well as the β rays which they produce: the whole effect is explained without hypothecating a new property. The same is to be said of the X rays; these produce cathode rays of various speeds in matter on which they fall. It is perhaps well to mention again that β rays and cathode rays are both electrons in motion, the only difference being that the former are going (at first) about three times as fast as the latter. Most of the measurements on this point have indeed been made with X rays, which are easier to handle: and the argument is carried over to the γ rays in accordance with the reliance we have learnt to place on their being exactly analogous.

Let us consider the way in which γ rays produce β rays or X rays produce cathode rays. In the first place we have the remarkable fact that the speed of the β rays depends on the quality of the γ rays and not at all on the nature of the material in which they arise. It is exactly the same sort of thing as we saw took place when β radiation was scattered by material on which it fell; only we cannot now say that the scattered radiation is merely the incident radiation scattered without change. Very penetrating γ rays cause fast β rays: as we take less penetrating rays of the one kind, we find the speed and penetration of the other diminish also; until we come to the "softest" X rays, when the cathode rays which they produce can only cross one or two tenths of a millimetre of air. But it does not matter what material we employ to bring about the change from one form of ray to the other; our choice does not effect the speed of the electron, which is the direct result of the change.

Now for a second point. When the γ rays pass perpendicularly through a thin plate it is found that the β rays having their origin in the plate are far more numerous on the side of the plate from which the γ rays emerge than on the side upon which they fall. It looks as if the γ rays drove the β rays

before them or as if the β rays to some extent took the place of the γ rays which have been absorbed in the plate. The dissymmetry is most marked in the case of the penetrating γ rays and is much less in the case of the X rays.

Just as in the case of the β rays in relation to the secondary or scattered β rays, so now we must try to settle one point before we go further. Whence comes the energy of these β rays which the γ rays produced? It is a most important question; the whole argument that follows depends on the answer we give to it. Shall we suppose the energy to come from the atom, so that the γ ray, so to speak, merely pulls the trigger and the β ray results from some intra-atomic convulsion which was on the point of happening and is now precipitated? Or shall we suppose the γ ray to provide the energy for the β ray and the atom to play no higher a part than that of energy transformer? The reasons in favour of the one or the other of these alternatives are practically the same as those which were before us in the corresponding case of the β ray. The whole argument seems to me to be now as then in favour of the latter of the two. It becomes easy to understand why the speed of the β ray and the direction of its motion are so closely connected with the γ ray from which it draws its energy: and why the atom itself, where the change takes place, is so unimportant an agent. It seems unnatural to suppose that the atom contains, as it were, an infinite number of guns pointing in all directions and that when the γ ray enters it fires off just that gun which gives a muzzle-velocity characteristic of that γ ray and which happens to point more or less in the direction in which the γ ray has been moving. On the other hand, it is not so difficult to picture the γ ray as carrying with it into the atom a certain amount of energy and momentum, and handing them on to an electron, which then proceeds from the atom as a β ray.

Supposing then that we are satisfied as to the answer we give to this fundamental question, we proceed to take into the argument another experimental fact. It is found that the speed of the β ray does not depend on the distance between the radium which is the source of the γ rays and the atom where the β ray comes into existence. No matter how far the γ ray has gone away from its origin, it hands over to the β ray exactly the same amount of energy.

Now there are two methods according to which radiation may spread away from a source and the two stand in apparent antithesis to each other. One of them may be illustrated by the spreading of the ripples on a water surface into which a stone has fallen: the disturbance is distributed over ever-widening circles and the height of the ripple diminishes accordingly. If some small object is floating on the pond, the movement which it experiences as the ripple passes under it is smaller the farther away it is from the splash. This is *not* the kind of spreading of radiation that we now require. We may illustrate the other method by picturing to ourselves a number of soldiers holding a small fort and shooting at enemies who are on all sides of them. Here the bullets radiate from a common centre and, of course, if we may suppose for the purpose of our illustration that the shooting is quite at random, the chance that a particular soldier of the enemy is hit is less the further he is away from the fort. But if he *is* hit the force of the bullet is practically the same as if he is close to. Hits are less frequent at greater distances but they are as severe in their results. It is this sort of radiation which the γ rays resemble. To distinguish it from the former we may term it "corpuscular"; and the use of the word would imply that we regard γ rays as a stream of discrete entities, a flight of projectiles each one of which keeps its characteristics, such as form and volume and energy, unchanged during its motion, until it spends energy in the production of the β ray. We need not at this stage define the corpuscle more closely than is necessary to this idea: we are not bound so far to say whether it is "material" or "immaterial" nor to try and find it amongst the things with which we are already familiar. It is hardly necessary to repeat that the X rays go with the γ rays: the two sets of rays differ only in degree and not in kind.

We may take one more step forward which tends to simplicity. As is well known, the X rays spring from a plate on which cathode rays are falling. It is found that the cathode rays which spring up under the action of the X rays have the same speed as the original cathode rays in the X ray tube. Perhaps we ought rather to say that we actually find some of them possessed of the same speed and we infer that the rest had it when they came into existence but have lost it, as indeed we should anticipate, before we can bring them to

measurement. Now there are great difficulties in the way of supposing that several cathode rays act together to make one X ray and that several X rays act together to make one cathode ray again. It would be like supposing that no injury was caused to person or thing by the bullets from the fort spoken of above, until several bullets had hit in the same place and that then the effect of all the bullets was added together. So the only thing left to us to suppose is that one cathode ray makes one X ray at the anticathode, giving to it most of its energy. The X ray passes out of the tube and through many atoms, until at last it meets its fate: it disappears and now we have a cathode ray again, inheriting the energy of the X ray and continuing its motion. It is a case of transformation and retransformation; and the thing that is transformed is the projectile that carries a definite quantity of energy. We are strongly tempted to speak of it as one thing throughout; just as we may think of the Australian mail as one thing though at one time the bags are carried by a train and at another by a steamer. It all depends what we fix our ideas upon.

It is when we try to be more definite as to the nature of the corpuscle of the γ or X ray that we find a difficult task before us and disagreement as to the course to be adopted. It is not that we must know whether it is material or immaterial; the point is rather, does the stream of corpuscles behave like a radiation working under Maxwell's laws of electromagnetism? If it does, it is rather a surprise to find such a thing to be possible; and our ideas of the radiation of light would be altered. We should, I think, come to the conclusion that we had been putting unnecessary and false details into our mental picture of it: forcing it to look to our minds like waves moving over the sea or through the air when we ought not to have given in to our cravings for complete models. We should be compelled also to admit that we were farther from a comprehension of the processes of interference and diffraction than we had hoped; and that, though we still had mathematical equations which drew all the phenomena together, we could not form a mental picture what the terms of the equations stood for. There are many physicists nevertheless who are willing to move in this direction, being reluctant to let the γ and X rays stand outside a scheme which shall link them up to electromagnetic radiations and to light; and they point

to the facts that among X ray phenomena there are one or two which are more readily expressed in terms of wave motion than of projectiles: whilst among light phenomena there are some which strongly resemble the effects of "corpuscular" motions. The X rays may be polarised in so far that a pencil may be more liable to deflection in one direction than in another; and the effect is qualitatively what a wave theory would lead us to expect. But the resemblance to polarised light stops short at this. Moreover, primary X rays can excite a secondary X radiation in certain substances which is less penetrating than the primary and is characteristic of the substance rather than of the exciting rays, reminding us of fluorescence in the case of light. On the other hand light can cause electrons to be ejected from substances on which it falls; they move very slowly and can be classified with the δ rays. According to recent experiment they seem to show the effect we have met with above; they continue to some small extent in the direction of motion of the radiation to which they are due.

These parallelisms are very interesting and striking; and when we consider them along with the simple and satisfactory way in which the great bulk of X and γ ray phenomena are explained by a corpuscular theory and the difficulties into which we are plunged by a corpuscular theory of light (it has been reckoned one of the great achievements of the nineteenth century to have escaped from them) we can only say that this section of modern physics is in a very interesting position. Are we to say that Newton and Huyghens were both right? Shall we some day be able to understand that corpuscular and wave radiations are only different aspects of the same thing? Or are we to adopt a different view; to say that α, β, γ and X radiations are all corpuscular, this common property explaining their common characteristics, that light as a wave motion stands apart and that the resemblances described above are but accidental? It works very well to suppose that the γ and X rays are only electrons with enough positive electricity added to them to neutralise their negative charge and so allow them to slip easily through electric fields inside the atoms which would have stopped them otherwise; the positive addition must be supposed to bring very little mass with it. On this view, the progress of the X ray is

that of an electron which as a cathode ray is without its positive, which picks it up at the anticathode and becomes an X ray and which may drop it again in passing through some atom. I venture to think that from this point of view the phenomena of the X and γ rays are very simple and easily grasped; and the hypothesis has been a good guide to experiment. Whatever be the outcome, this hypothesis will be found, I think, to contain truth.

These paragraphs are in a sense a digression from my main procedure, which was to show that γ and X rays illustrated the penetrability of matter. But it was not possible to show the force of this until something had been said of the properties of these rays. When we realise that so many things point to a corpuscular form, whether the corpuscle be "material" or not, it becomes singularly interesting that they can proceed in straight lines through such great quantities of matter. Let us suppose, if we like, that atoms contain electrons only, each having a radius of 10^{-13} cm., the value usually assigned; we shall find if we make the calculation that a γ ray will go directly through many electrons in crossing an inch of steel. Then what becomes of the idea of the impenetrability of matter? It has indeed been slowly vanishing away. At one time atoms were impenetrable; we put into our visions of them ideas of contact and surface blows borrowed from our observations of colliding balls. Then radioactivity came and the very first experiments were enough to break down the crude idea. As we go further we see no reason for a halt at any particular point and we incline to the belief that there is nothing in the universe with which we are acquainted which possesses the sole right or power to occupy any particular portion of space to the exclusion of everything else. Relative speed and enough of it is all that is wanted to make penetration possible to the last degree.

THE CHEMICAL HISTORY OF THE TERPENES

A CHAPTER IN MODERN SYNTHETICAL CHEMISTRY

By SIR WILLIAM A. TILDEN

The hydrocarbons known as the terpenes have long attracted the attention of chemists, on account of their wide diffusion in the vegetable kingdom and their frequent association with the odorous principles of plants, though it is only during the last thirty to forty years that systematic research has been rewarded with such a measure of success that it can be said that the properties and chemical constitution of all the more important members of the group are now as well understood as those of any other group of carbon compounds.

The terpenes are highly interesting from two distinct points of view. As already mentioned, they are important for commercial reasons not only on account of the use of some of them as solvents, especially the oils of turpentine in varnishes and paints, but they are characteristic and often predominant constituents of many essential oils used in medicine and perfumery. They have also been made the starting-point for the manufacture of certain substances—camphor and terpineol, for example, which are extensively used for such purposes.

But the history of the investigations by which the chemical constitution of many of the terpenes has at last been elucidated is specially interesting to the scientific chemist on account of the nature of the problems to be solved, the peculiar elusive transformations of the hydrocarbons and their derivatives and the ultimate success of these investigations, which provides a triumphant vindication of the principles which underlie modern synthetical chemistry. These principles concern not only the student of "organic" chemistry, for they are necessarily involved in general conceptions regarding the constitution of matter.

For the purposes of this article it will be convenient to restrict the name "terpene" to the unsaturated hydrocarbons having the empirical formula $C_{10}H_{16}$, of which oil of turpentine and essence of lemon may be regarded as typical examples. All, with one exception, are liquids having characteristic odours, lighter than water and soluble in that liquid only to an extent sufficient to communicate their peculiar taste and smell. Nearly all the natural terpenes are optically active, some rotating the ray to the right, others to the left.

A few of the derivatives of turpentine were known long before any means were available for determining their composition. Thus terpin hydrate, in the form of crystals deposited from old specimens of turpentine oil, was described nearly two hundred years ago; moreover, the hydrochloride, which was at first confused with camphor, was obtained by Kind in 1803[1] by passing hydrogen chloride gas into turpentine oil. This was before chlorine had been recognised as an element and when the nature of muriatic acid was unknown.

The first definite knowledge of the composition of terpenes was provided by Dumas and by Blanchet and Sell, who in 1833[2] analysed, by Liebig's then new process, both oil of turpentine and essence of lemon and found that, in spite of a difference of some 20° in boiling point, these hydrocarbons had the same composition.

Twenty years later Berthelot studied the two commercial varieties of turpentine—French and American—and the action of heat upon them[3] but then and for many years after it was supposed that there was an indefinitely large number of hydrocarbons of the same composition, $C_{10}H_{16}$.

The first classification attempted was based on the production, properties and interactions of the nitrosochlorides which are formed by the union of the terpene with nitrosyl chloride, NOCl.[4] The three classes then recognised were, however, found by further research to be insufficient and as explained later the number of hydrocarbons of distinct constitution is very much greater than was then supposed. The nitrosochlorides, however, have played a very important part in the

[1] *Tromsdorff's J. d. Pharm.* xi. pt. ii. 132.
[2] *Annalen*, **6**, 245 and 262.
[3] *Annales de Chim. Phys.* [3], **39**, 5.
[4] Tilden, *Chem. Soc. Trans.* 1875, 1877, 1878.

inquiry, as they are highly active substances and have provided the starting-point for many series of derivatives which would otherwise have remained unknown.

Since 1885 the number of researches into the characters and reactions of the terpenes and their derivatives has greatly increased but while many have supplied facts relative to individual series, the most important systematic inquiries which have led to the present advanced state of knowledge of these compounds are those of O. Wallach,[1] A. von Baeyer,[2] G. Wagner,[3] and W. H. Perkin, jun.[4]

The principal steps in the process of inquiry, taken in approximately chronological order, are as follows. In 1856 it had been shown by Greville Williams that turpentine produces cymene by loss of two atoms of hydrogen.[5] In 1872 it was announced by A. Oppenheim that oils of turpentine and lemon combine with bromine and that when heated with aniline their dibromides lose two molecules of hydrogen bromide and yield common cymene.[6] C. R. A. Wright also showed that the cymenes obtained from different sources, including camphor and the terpenes, are identical.[7] From these and other facts the terpenes were assumed to be dihydrocymenes. Cymene was then known to be methyl-*para*-propyl-benzene; it was only after a long controversy, in which several chemists took part, that it was ultimately shown by O. Widman[8] that ordinary cymene contains *iso*-propyl and not normal propyl, as had been previously supposed.

But much difficulty was felt in accepting Oppenheim's formula for the terpenes owing to their peculiar properties, for this formula was proposed at a time long antecedent to Baeyer's important papers on hydrobenzene derivatives. No benzene derivative was at that time known to combine directly with hydrogen chloride for example, and accordingly Flawitzky and others were led to propose open-chain formulæ for the terpenes as more in harmony with their quasi-aliphatic character.

A clue was obtained in 1885, when, in extending the study of the oximes, a class of compounds recently discovered by

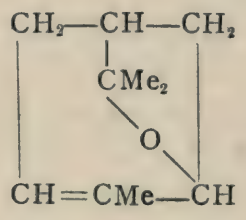
[1] *Annalen*, 225, etc.
[2] *Berichte*, 26, etc.
[3] *Ibid.* 27, etc.
[4] *Chem. Soc. Trans.* 1904, etc.
[5] *Proc. R. Soc.* **10**, 516.
[6] *Berichte*, **5**, 628.
[7] *Chem. Soc. Trans.* [1873], 686.
[8] *Berichte* [1891], **24**, 439.

Victor Meyer and Janny, it was shown by Goldschmidt and Zürrer[1] that the nitroso-hesperidene obtained by Tilden and Shenstone from the nitrosochloride of the terpene contained in orange oil was identical with carvoxime, $C_{10}H_{14}:N.OH$. Difficulties, however, still remained, as the nitrosopinene obtained by a similar process from turpentine oil could not be similarly identified. Fortunately about this time Baeyer had begun those researches into questions connected with the constitution of benzene which were destined later to shed much light on these obscure problems. The products of the reduction of terephthalic acids were shown to have properties very different from those of ordinary benzene derivatives and to approximate closely to those of fatty compounds. The di- and tetra-hydro derivatives combine directly not only with bromine but with hydrogen bromide, while the hexhydro derivatives are saturated. The difficulties involved in the acceptance of Oppenheim's formula for the terpenes were thus partly dispelled, as it appeared that the addition of two atoms of hydrogen to cymene was capable of modifying profoundly its chemical behaviour. Nevertheless the formula, while suggesting the *para* constitution since found to be so prevalent among the terpenes and the compounds naturally associated with them, was manifestly not equally applicable to both the principal classes of terpenes then known possessing different physical and chemical properties.

In 1889 a further important step forward became possible as a consequence of the discovery by G. Wagner[2] that the first product in the oxidation of many and perhaps all unsaturated compounds by means of neutral or alkaline permanganate is a hydroxy compound, which is formed by the addition of hydroxylic groups at the double bond, where rupture occurs on further oxidation. Thus the olefines yield the corresponding glycols, and alcohols of the allyl series yield glycerols under the action of this agent, and the number of double linkages in a molecule could, in fact, be determined by studying the products of oxidation under the prescribed conditions. It was found, for example, that camphene yields a glycol $C_{10}H_{16}(OH)_2$ and limonene a tetrahydric alcohol $C_{10}H_{16}(OH)_4$. The oxidation of pinene by acid agents is misleading owing to the isomeric change which is brought about by contact with aqueous acids, so that dipentene derivatives alone are formed. Wagner at this time suggested for

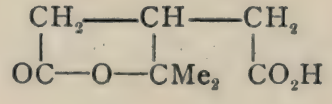
[1] *Berichte*, **18**, 2220. [2] *Ibid.* **21**, 1230; **21**, 3347; **23**, 2307.

pinene a formula with a *para* linkage, still containing the propyl group. But by 1894 he was led to a different expression as a consequence of studying the oxidation of pinole, $C_{10}H_{16}O$, which is obtained by treating with dilute acids the compound called sobrerol in honour of its discoverer Sobrero.[1] This is a glycol formed by exposing pinene to contact with oxygen and water in the presence of sunlight, also by oxidation of pinene by means of mercuric acetate, and among the products of the action of nitrosylchloride on pinene. For pinole Wagner proposed the following constitutional formula:

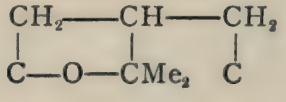


By careful oxidation of pinole with permanganate Wagner obtained first a quantitative yield of the glycol $C_{10}H_{16}O(OH)_2$ which in accordance with his rule indicated the presence of one ethylenic bond in the molecule. Terpenylic acid was found to be the final chief product.[2]

Terpenylic acid, discovered by Hempel in 1875, had already been shown by Schryver[3] to be a lactone of the following structure:



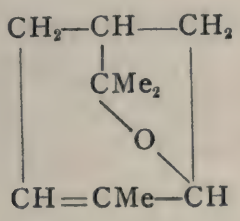
It may be assumed that the pinole from which terpenylic acid is produced already contains the grouping

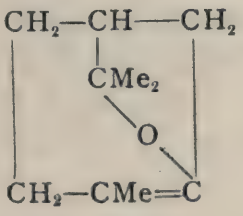


and the question only remains how this is associated with the residue CMe and the two hydrogen atoms which have been removed.

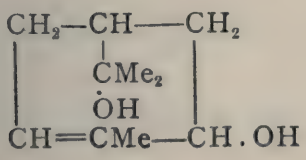
[1] *Compt. Rend.* [1851], **33**, 66.
[2] *Ber.* [1894], **24**, 1644.
[3] *Trans. Chem. Soc.* [1893], 1327.

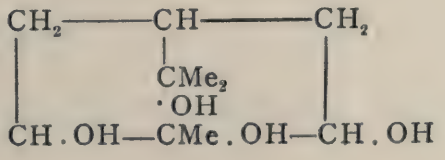
The two following formulæ present themselves of which the second is found to be excluded.

I. 

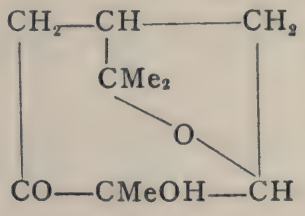
II. 

Pinole is an unsaturated compound and the hydrate, sobrerol, when oxidised by permanganate yields a tetrahydric alcohol $C_{10}H_{16}(OH)_4$ (m.p. 155·5°–156°) the production of which is in harmony only with Formula I. given above. Sobrerol and the tetrahydric alcohol being respectively



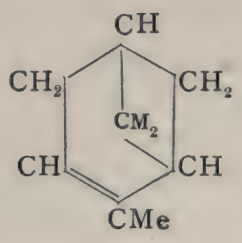


the successive stages of the oxidation of pinole can be easily traced. The glycol $C_{10}H_{16}O(OH)_2$ first formed passes into the ketol



and this ultimately into terpenylic acid and acetic acid.

The formula for pinole may therefore be regarded as established on a firm basis, and, with remarkable insight, Wagner perceived that an expression for pinene directly derived from it would be found to supply a satisfactory explanation of the hitherto mysterious behaviour of that hydrocarbon.

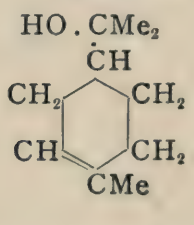


Pinene is thus represented as a bicyclic compound which includes a dimethylated tetramethylene ring.

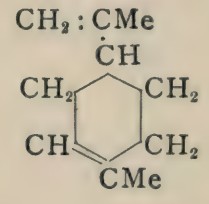
The applicability of this formula in other directions may now be considered.

The production of crystals in moist specimens of oil of turpentine has long been known and the composition of the substance thus formed was established by Dumas and Péligot in 1834.[1] The compound terpin hydrate contains the elements of the hydrocarbon united with two molecules of water and a molecule of water of crystallisation, $C_{10}H_{20}O_2 . H_2O$. It is a glycol which loses the elements of water very readily and yields terpineol, an unsaturated monohydric alcohol $C_{10}H_{17}OH$. Terpineol in its turn yields, by loss of water, dipentene (racemic limonene) $C_{10}H_{16}$, a hydrocarbon isomeric with pinene which is freely converted into it by contact with dilute aqueous acids. Terpineol and dipentene (limonene) are now known to have the constitution assigned to them by Wagner[2] and these formulæ result from the hydrolytic disruption of the tetramethylene ring.

Terpineol.



Dipentene.

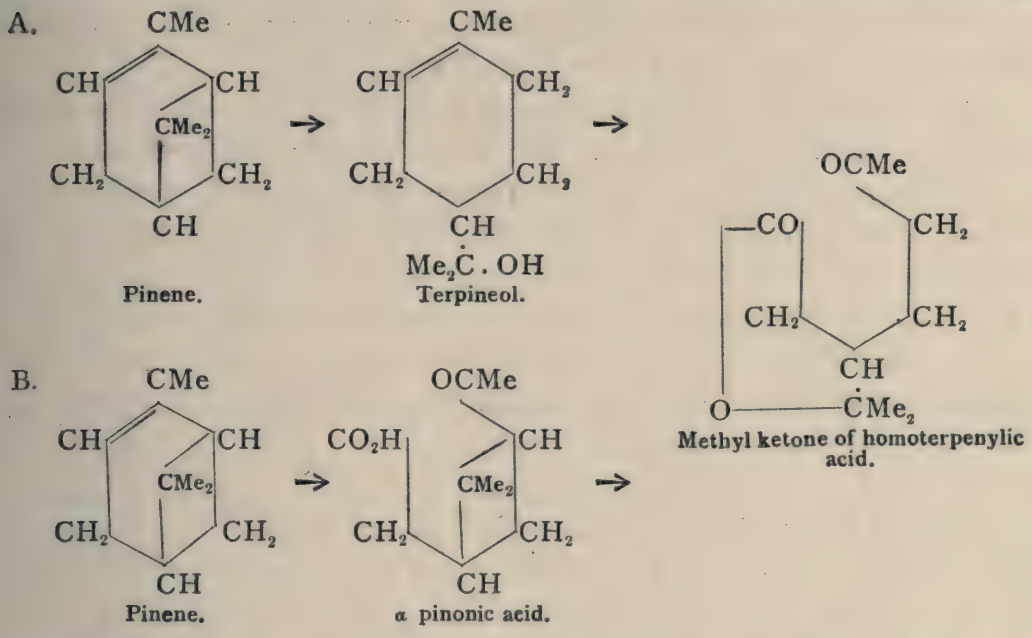


That these formulæ have been for some years generally accepted by chemists is largely due to the timely support given to Wagner's views by the researches of Baeyer.

By the oxidation of terpineol Baeyer showed in 1896[3] that a product is obtained, namely the methyl ketone of homoterpenylic acid, which is identical with the product of hydrolysis of *a* pinonic acid, and that acid is produced by the direct oxidation of pinene by means of neutral permanganate. Starting from pinene in both cases, therefore, the operations required are either hydrolysis and subsequent oxidation (A) or oxidation and subsequent hydrolysis (B).

These processes are represented by the Wagner formulæ as follows:

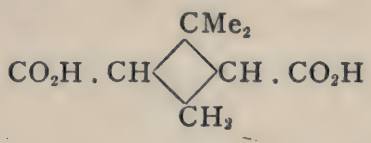
[1] *Annales Chim. Phys.* **57**, 334.
[2] *Loc. cit.* [3] *Ber.* **29**, 2775.



a pinonic acid is converted by heating with 50 per cent. sulphuric acid into the ketone, which was first obtained by Wallach as a product of the oxidation of terpineol.

The dimethylated tetramethylene ring in the formula for pinene has been called by Baeyer " picean," and he has directed special attention to its peculiarities. While it is readily ruptured by the hydrolysing action of dilute acids, it resists the attack of neutral oxidising agents in a remarkable manner.

Among the products of the oxidation of pinene by permanganate is found not only *a* pinonic acid $C_{10}H_{16}O_3$ but a dibasic pinoyl-formic acid $C_{10}H_{16}O_5$. Both these compounds contain the same stable nucleus of four carbon atoms, which is capable of hydrolytic rupture much as in the case of pinene itself. Both acids when further oxidised yield the same pinic acid, $C_9H_{14}O_4$, which, however, is not similarly changeable by hydrolysis. Pinic acid when brominated and then treated with baryta gives a hydroxypinic acid, from which by further oxidation norpic acid, $C_8H_{12}O_4$, is obtained. Norpic acid has the constitution shown below, retaining the picean ring:

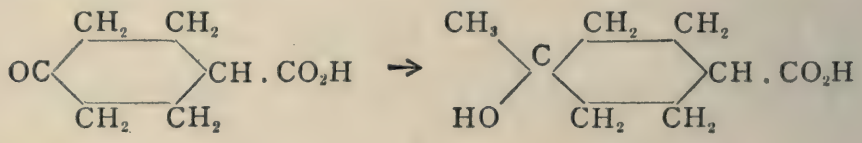


Norpic acid is extremely stable and resists further oxidation.

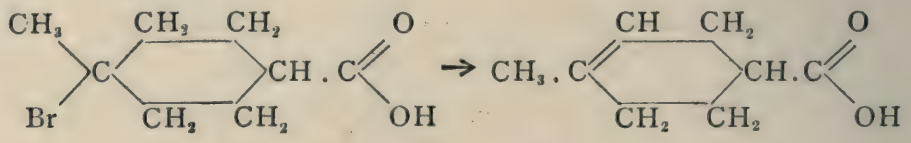
Finally, the view of the constitution of terpineol set forth

above has been completely established by the brilliant synthetical researches of W. H. Perkin, jun.[1]

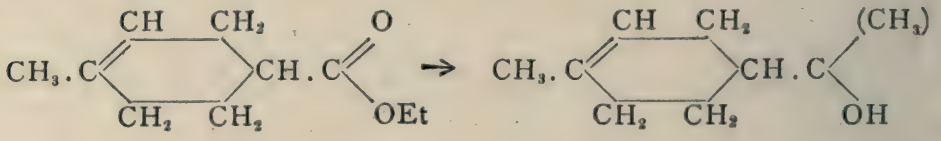
Perkin's synthesis proceeds as follows: In the first place, the esters of β iodopropionic and sodiocyanacetic acid are caused to interact and the product is hydrolysed; pentane-$\alpha\gamma\epsilon$-tricarboxylic ester is thus obtained, from which δ ketohexhydrobenzoic acid is formed by its treatment with acetic anhydride and distillation, carbon dioxide and water being eliminated. The ester of this acid, in contact with magnesium methyl iodide, yields a product from which δ hydroxyhexhydro-*p*-toluic acid is obtained by hydrolysis.



Hydrogen bromide converts this toluic acid into the corresponding bromo-derivative, which when treated with weak alkalis or pyridine loses hydrogen bromide and yields Δ^1 tetrahydro-*p*-toluic acid.



The ester of this unsaturated acid is converted by magnesium methyl iodide in ethereal solution into inactive *terpineol.*



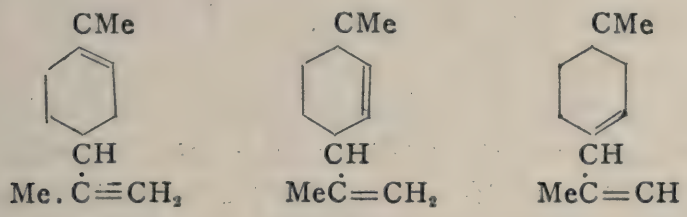
This is Wagner's formula for terpineol and since dipentene (limonene) is obtained from terpineol by a simple operation of dehydration the position of the second Δ or ethylenic bond in the hydrocarbon is determined.

Optically active *d* and *l* terpineols were afterwards prepared by Fisher and Perkin.[2]

It now becomes obvious that the formulæ of some of the other natural terpenes may be sought first of all among the

[1] *Trans. Chem. Soc.* [1904], 654.
[2] *Ibid.* [1908], 1871.

isomerides of limonene, in which the double bond is otherwise placed. Theoretically there may be three such compounds in which the Δ is in the ring—



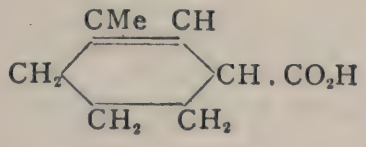
and some ten or twelve others all having the *para* distribution of the side chains.

It is somewhat remarkable that the great majority of the terpenes occurring in nature belong to this type, for, as already mentioned, most of them may be converted into *p* cymene by comparatively simple operations.

Terpenes of the ortho- type have not at present been recognised among natural terpenes, but a dihydro-*o*-terpene or *o* menthene has been prepared synthetically by Ray and Perkin.[1]

The idea that terpenes may exist in which the meta position is occupied by the side chains has also been put to the test by Perkin, who has succeeded in producing synthetically not only a number of compounds of this structure, but one hydrocarbon which is identical with the sylvestrene originally isolated from Swedish turpentine by Atterberg in 1878.[2] The method used was similar to that which has already been described in the case of the *para* compounds.

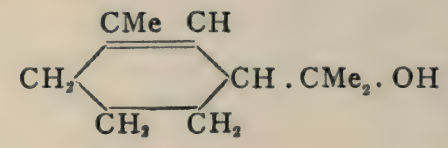
Proceeding from Δ^1 cyclohexene 3 carboxylic or Δ^1 tetrahydro-meta-toluic acid,



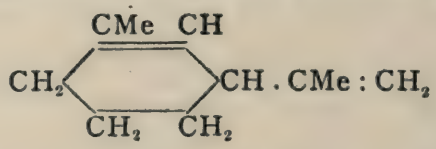
the *d* acid was first isolated from the inactive mixture by means of brucine and the ester made to interact with magnesium methyl iodide. The resulting *d* dihydrocarvestrenol is the

[1] *Trans. Chem. Soc.* [1905], 1066.
[2] *Ber.* **10**, 1202.

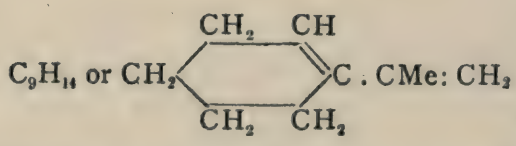
representative of terpineol in the meta series, and is expressed by the following formula:



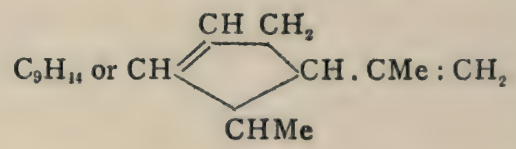
When treated with hydrogen chloride it is converted into *d* carvestrene dihydrochloride which melts at 72° and is identical with the hydrochloride formed by natural sylvestrene. The hydrocarbon is isolated from this compound by heating it with aniline; it is represented by the formula [1] given below:



Perkin has also made some advance in the production of lower homologues of the terpenes. The hydrocarbons prepared up to the present belong to two types. One of these is derived from cyclohexene containing no methyl group in the ring [2]:



The other is framed on a cyclopentene containing a methyl group [3]:



Both these substances are liquids possessing a lemon-like odour, and they absorb oxygen with great rapidity in contact with air.

A hydrocarbon which has the composition C_9H_{14} exists in the lower fractions of the oil from sandal wood, but its constitution must for the present be regarded as unsettled.

It is unnecessary to pursue this part of the subject further. It is sufficient to point out that the natural terpenes, $C_{10}H_{16}$, so

[1] *Proc. Chem. Soc.* [1910], 97.
[2] Matsubara and Perkin, *Trans. Chem. Soc.* [1905], 661.
[3] Haworth and Perkin, *Trans. Chem. Soc.* [1908], 573.

far as they have been investigated, range themselves under the following divisions:

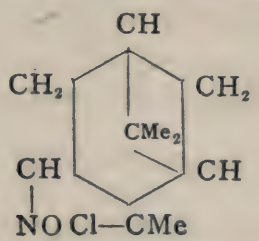
MENTHADIENES. $C_{10}H_{16}$.

I. Derivatives of cyclohexene:
   1. *Para.* Limonene and dipentene, terpinene, phellandrene.
   2. *Meta.* Sylvestrene.

II. Derivatives of cyclohexene with bridged rings:
   1. *Para*, containing two pentamethylene rings. Fenchene, camphene?
   2. *Meta*, containing one tetramethylene ring. Pinene.
   3. *Meta*, containing one trimethylene ring. Sabinene, thujene.

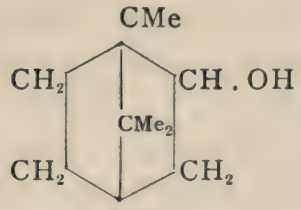
III. Olefinic terpenes with open chains:
   Myrcene, ocimene.

A few of the peculiarities of some of the terpenes deserve notice. As already mentioned, pinene is specially remarkable on account of the readiness with which it undergoes isomeric change in contact with almost all agents. This is attributed to the rupture of the tetramethylene ring, whereby cyclohexene derivatives such as terpineol and dipentene are produced. The products of addition present the anomaly that the hydrocarbon cannot be recovered unchanged from any of them with the single exception of the nitrosochloride. That substance, when warmed with an alcoholic solution of aniline, gives the diazo-reaction and behaves as though it contained ready-formed nitrosyl chloride, whilst pinene, identical with the original hydrocarbon in all respects except in being destitute of optical activity, is set free. If methyl- or dimethyl-aniline be used, a nitroso derivative of the base is produced. From these facts it appears that when the nitrosochloride is formed the addition of NO and Cl occurs at the double bond in the hydrocarbon molecule which remains otherwise unaltered, thus:

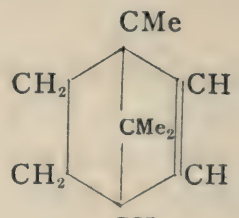


The case is altogether different when hydrogen chloride is concerned. The product of the union, the long-known "kunst-camphor," is an unusually stable substance which is volatile without appreciable decomposition, and from which the chlorine is removed with difficulty by alkali or alkaline salts. This substance appears to be identical with bornyl chloride, which, together with isobornyl chloride, is formed by the action of phosphorus pentachloride on borneol.[1]

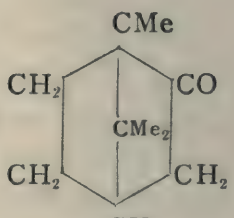
From that compound a hydrocarbon, bornylene, is obtained which stands in a natural relation to borneol and to camphor.



Borneol.

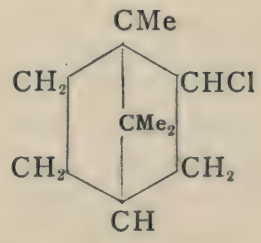


Bornylene.



Camphor.

The chloride must therefore be—



and its production from pinene must be the result of considerable isomeric change, the exact course of which can only be guessed at.

The constitution of pinene dibromide is also a matter of doubt. According to Wallach, on the one hand[2] it yields cymene when hydrogen bromide is withdrawn by heating it with aniline; on the other hand, it is converted by reduction into a dihydro-camphene identical with that which is derived from pinene hydrochloride.[3] Both these changes involve internal rearrangement, whatever formula is assumed from the bromide.

Another question of some interest is involved in the obser-

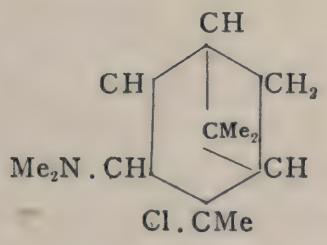
[1] Wagner and Brickner, *Ber.* [1899] **32**, 2302; [1900] **33**, 2121.
[2] *Annalen*, **264**, 1.
[3] Semmler, *Berichte* [1900], **33**, 3420.

vation of Baeyer that the nitrosochlorides are generally bimolecular—that is, the molecule consists of $(C_{10}H_{16}NOCl)_2$.[1] The production of an intense blue colour during the process of combination and before the compound begins to be deposited in the crystalline form is a sign that the compound first formed is a nitroso compound and has not assumed the constitution of an oxime. If the white crystalline nitrosochloride is dissolved in chloroform or benzene, a faint reproduction of the same colour is observable. These facts, together with its behaviour with aromatic bases, already referred to, afford proof that the NO group is present as such and that the hydrogen attached to the same carbon atom in pinene has not left the carbon to produce : N . OH. It is interesting to observe that when the chlorine atom is displaced by CN, by NH_2 or by N_3, the link between the two molecules of nitroso compound is unloosed and the compounds formed are *uni*molecular, while the nitroso compound becomes an oxime.

The union between the two constituent molecules in the *bis*nitrosochloride appears to be due to the oxygen,

$$C_{10}H_{16}Cl\,.\,N:O:O:N\,.\,C_{10}H_{16}Cl,$$

which, as in so many other cases now recognised, exerts four units of valency. That the union is not effected by the nitrogen or the chlorine is indicated by the fact that when these two elements are retained and the oxygen displaced the compound is again *uni*molecular, as in the base represented by the following formula[2] :



The bisnitrosochloride is optically inactive and is probably racemic in character and if the two semi-molecules could be separated they would probably be found to exhibit respectively dextro and lævo rotatory power. It has been found that a greatly increased yield of the nitrosochloride is obtained when,

[1] *Berichte* [1895], **28**, 639 and 652.
[2] Tilden and Stokes, *Trans. Chem. Soc.* [1905], 836.

instead of operating upon ordinary dextro- or lævo-pinene, an optically inactive mixture of the two hydrocarbons is used.[1]

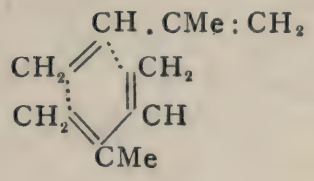
The action of heat on the terpenes, long ago studied by Bouchardat, Berthelot and other chemists, has led to results which have become of some practical interest within recent years in connection with the efforts which are being made to produce caoutchouc or rubber artificially. When pinene is heated to about 250° its rotatory power is reduced and ultimately destroyed, a part of the hydrocarbon being converted into dipentene and part into products of polymerisation. If the vapour of pinene, limonene or dipentene is heated to a temperature of about 400° it is partly resolved into a pentine, C_5H_8, which is identical with one of the products of the action of heat on indiarubber and is known as isoprene. It has been recognised as β-methylcrotonylene, $CH_2 : CH . CMe : CH_2$.[2]

Isoprene heated alone in a sealed tube is converted into dipentene mixed with viscous products of its polymerisation. But if it is kept for some months or years under conditions which, except exclusion of air, have not been exactly determined, it passes spontaneously into a syrupy liquid which gradually deposits solid masses of rubber. Rubber is also formed when isoprene is heated with small quantities of acetic and other acids. This observation, made more than twenty years ago, has led to various attempts to employ isoprene as a practical source of rubber on a manufacturing scale.

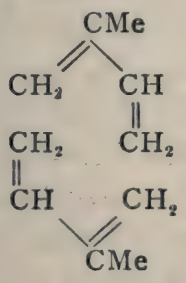
When rubber or gutta-percha is destructively distilled it yields a mixture of hydrocarbons from which can be separated as chief products, isoprene and dipentene, originally called, caoutchine.[3] It would appear from this that rubber is a compound, the formula of which is either $(C_{10}H_{16})_n$ or $(C_5H_8)_n$. From the direct conversion of isoprene into rubber the latter formula seems the more probable, especially as the temperature at which rubber decomposes is much below that at which dipentene is broken down under the influence of heat. Indeed, it seems not improbable that the dipentene which accompanies isoprene in the distillation of rubber is a secondary result of the polymerisation of isoprene by heat. The condensation of isoprene into dipentene is easily represented as in the following formula

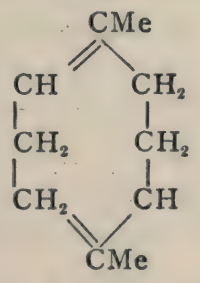
[1] Tilden, *Trans. Chem. Soc.* [1904], 759.
[2] Mokiewsky, *Chem. Cent.* [1899], i. 589.
[3] C. Greville Williams, *Proc. R. Soc.* [1856], **10**, 516.

where two molecules of isoprene are united by the dotted lines, the valencies adjusting themselves but without further change:



To represent the condensation of isoprene into caoutchouc, Harries[1] assumes the formation of an octadiene as the first product of the union of two molecules of isoprene, rubber consisting of multiples of this or $(C_{10}H_{16})_n$.





Caoutchouc is a colloid of very high molecular weight and if it is assumed to be made up of a number of such groups united together, it is difficult to explain why rubber should break down so easily in groups of C_5H_8 and why its combining capacity is represented by four atoms of bromine for every ten atoms of carbon.

Thiele's theory of partial valencies has been invoked by way of explanation but this is itself a subject of extremely doubtful character.

Other formulæ have been suggested but the whole question is one which requires much further investigation.

Finally, it may be remarked that the terpenes and their derivatives afford many examples of optically active compounds which, however, do not owe their activity to the presence of one or more asymmetric carbon atoms but rather to the asymmetry of the molecule as a whole. This phenomenon, however, is not peculiar to the terpenes and the question is too large for discussion on this occasion.

[1] *Berichte* [1905], **38**, 3985.

SOME ASPECTS OF PERIODICITY IN PLANTS

By PERCY GROOM

The successive morpholigical changes that collectively constitute the ontogeny of an individual exemplify but one of the many manifestations in protoplasm of periodic action, protoplasmic periodicity being often revealed in rhythmic physiological activity almost or entirely unaccompanied by appreciable change in structure. Clear cases of such periodicity are afforded by unicellular organisms, in which various forms of functional activity wax and wane in due order: for instance, beginning with rhythmic contractility leading to locomotion associated with irritability that constrains the organism to move in a definite direction; then passing on to special secretory activity that brings into being a cell-wall, whose formation may be succeeded by obvious growth and rapid photosynthesis, which in turn may give way to special kinds of contractility or secretory activity coincident with the assumption of a reproductive or resting phase. During these more or less obvious phases there is often, at least, invisible rhythmic change of such a nature that one and the same stimulus is succeeded by responses that differ with the age of the cell: such changed behaviour in response to stimuli of light and gravitation are especially familiar among naked mobile masses of protoplasm of Algæ and Myxomycetes. To what extent this rhythmic functional activity of the protoplasm may ultimately be expressed in physico-chemical terms as simple as those explanatory of the rhythm of interacting chemical bodies or of enzyme-action, it is impossible to say. Rhythmic activity in the simple plant or cell may be autonomic or induced and is subject to modification by external factors. Klebs' work on the induction of reproduction in Algæ and Fungi especially exemplifies such change in the length of some of the phases constituting a complete rhythmic cycle; whilst the ether treatment and other methods of forcing, also the changed phenology of plants transferred to different altitudes or latitudes, illustrate analogous changes induced in Higher plants.

The induction of a special rhythm by intermittent geotropic and heliotropic stimuli was accomplished by F. Darwin and Miss D. Pertz. In highly organised plants the difficulties of interpreting periodic action are great because, on the one hand, if the rhythm be inherent it may be innate in the protoplasm itself or may arise from the general architecture and consequent limitation in mode of action or may result from both causes; whilst, on the other hand, if the rhythm be induced the difficulty in analysing the phenomenon is proportionate to the complexity of the plant and the ever-changing environment.

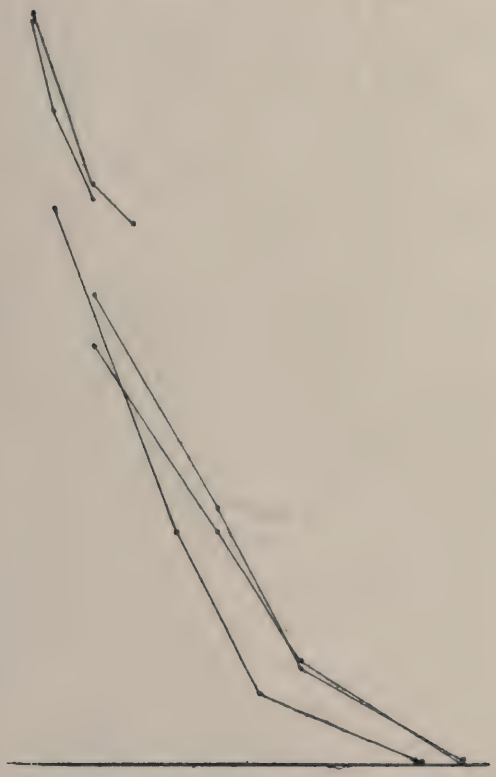
The lengths of the periods under discussion vary from a fraction of a second, in a moving cilium, to days, months or years, as represented by the longevity of a cell or organism.

The subject of longevity itself offers many problems for solution, even along paths that are strewn with facts already known. Very familiar is the fact that in many species of plants, varying from simple to the most complex, death is associated with a special form of reproduction. This is so much the case with phanerogams that if individuals of monocarpous species be prevented from flowering, their length of life may be prolonged apparently indefinitely (?); it is thus that the Chinese preserve the lives of monocarpous bamboos. Between polycarpous and monocarpous species various transitions occur. It is a familiar fact that our ordinary trees have seed-years succeeded by several more or less barren ones, during which, according to Hartig, reserve material is accumulating for the next effort. Here the explanation given of seeding is that it is postponed until a certain minimum of starch and other food-material is stored. Many shrubs and certain Indian bamboos have branches that inevitably die after fruiting. If all the branches of the bamboo thus flower and die to the ground, the plant is so weakened or depleted of food that in the following season it either sends up slender seeding-like shoots or is exterminated. Such species lead on to monocarpous types, if such absolutely monocarpous bamboos do actually exist. The cause of death in polycarpous plants is quite unknown, indeed it is impossible to say whether or no a tree ever dies of old age. It is certain that isolated plant cells and organs, for example, sexual cells, pollen-grains and seeds, perish naturally, that is to say, unattacked by parasitic foes. But the lives of cells (pollen grains) and organs (seeds) may be prolonged by appropriate

treatment. As seeds have been investigated from this point of view, some points concerning their longevity may be considered here. Exposed to ordinary conditions, with free access of air, the seeds of different species vary widely in their length of life. Very short-lived are those of some kinds of poplars and willows. In a set of trials cited by Wiesner, the fresh seeds of *Salix purpurea* germinated within twenty-four hours, whereas older seeds required from two to four days to initiate germination: of these seeds kept for forty days 21 per cent. germinated, kept for sixty days only 10 per cent., and kept for eighty-five days only 8 per cent. The results of the two tests of vitality of the seeds, namely time required for the inception of germination and the percentage of those capable of germinating, together suggest that death is no sudden catastrophe but is a gradual process of weakening. Before attempting to explain this gradual collapse, additional evidence pointing in the same direction may be quoted. In Döbner and Nobbe's *Botanik für Forstmänner* there are given statistics as to the percentages of seeds germinating after these had been kept for periods varying from half a year up to twelve years. The species tested included *Pinus sylvestris* and *Trifolium pratense*. The results are given in the form of percentages of germination in seeds of the same sample but of different ages. If the statistics be arranged in the form of curves, whose ordinates represent the percentages and whose abscissæ note the ages of the seeds, the resultant "curve of mortality" is a descending one having a primary steep descent succeeded (excepting in one sample of *Spergula arvensis*) by a gradual shallow concluding fall. The regularity of the descent is such as to indicate a steady influence at work killing out the seeds (for I assume that the investigators eliminated the possible source of error by determining the penetration of water through the testa). The curves printed here are those obtained from different samples of seed of *Pinus sylvestris*.

What is the gradual lethal influence? Some interesting experiments conducted by Giglioli (see *Nature*, 1895, p. 54) provide the first steps towards solution of the problem. Giglioli kept resting seeds in hermetically sealed tubes that contained various dry gases, including such poisons as chlorine, arseniuretted and sulphuretted hydrogen and carbon monoxide, also

in poisonous alcoholic solutions, including absolute alcohol and corrosive sublimate. He showed that seeds (especially those of *Medicago sativa*) could live in these poisonous surroundings during the whole time of his experiment, namely fifteen to sixteen years, but that even small traces of water led to the death of the seeds. These experiments prove, on the one hand, that dry seed-coats may be impervious to dry gases and to alcoholic solutions; on the other, that seeds can live for years under conditions that almost or entirely exclude respiration



Mortality of seeds of Scots pine.

(except in the form of infinitesimal intramolecular respiration). Likewise interesting were the observations of P. Becquerel, who found that various kinds of seeds, belonging to fifty species, had retained their vitality after having been kept in a museum during twenty-five to eighty-five years. He showed that these long-lived seeds had one character in common, namely an impervious, even stony, covering. P. Becquerel also found that when the seed-coat of a dried seed was artificially perforated and the seed was subsequently kept in high vacuo for two years it preserved its vitality. These various observations suggest that the death of seeds exposed to the air is due to access of water and oxygen.

In this relation some experiments described in Döbner and Nobbe's already cited work are of interest. Nobbe immersed 800 seeds of *Robinia Pseudacacia* in distilled water and observed the number of seeds that swelled in each of the successive years up to seven. He found that at the end of the time there were 134 still unswollen, but that these were living and germinated when the seed-coat was injured. This not only demonstrates that lack of germination of samples of seeds is often due to lack of penetration of water, not to death, but also affords a means of comparing the longevity and the impermeability of the seed-coat of seeds. For if death be indirectly due to the permeability of the seed-coat, then the seeds that swelled in each year would have been the ones to die if water had reached them (from the air, for instance) but not in sufficient amount to permit of growth. Hence the curve denoting the number of seeds unswollen after the increasing numbers of years ought to be of a form similar to that recording the number of seeds germinating after they had been kept in air during increasing numbers of years. The curve denoting the percentage of unswollen seeds in *Robinia* does conform with this anticipation, as may be seen by constructing it on the basis of statistics supplied by Döbner and Nobbe, which are as follows in percentages: seeds not swollen after ten days but swelling after one to seven years respectively, 55, 18, 7, 8, 4, 3, 3, 1. In this sample of 400 seeds, seventy-one swelled within the first ten days, while sixty-seven remained unswollen after seven years. It would, however, be more instructive to have statistical work done on the longevity and permeability of the seed-coat of one and the same kind of seed: with such work I am unacquainted.

The rhythmic morphological activity of phanerogams is expressed in the succession of different kinds of leaves not only in the life of the annual individual, but also in the growth of the perennial during one season, when foliage is succeeded by scales, and by flower and fruit. This order of succession is, however, liable to exception, as, for instance, in those species that burst into blossom before they unfold their leaves. The fact that different species occupying the same habitat produce leaves and flowers at different but more or less characteristic times is a sufficient indication that this periodicity is largely innate. Yet external factors can affect the rhythm by modifying the times of commencement and conclusion and the duration of the cycle, so that

the phenology of a species varies within limits with site, latitude and altitude. The causes of the inherent phenological differences have been but little investigated; in some cases at least, genetic affinity and the original geographical source of the species appear to be of importance. For example the relatively late seasonal flowering of the Sweet chestnut and of the lime, when compared with the majority of British trees, suggest quite correctly that these belong to a more southern home and have their blood-relations in warmer countries. But does the early flowering of the Common ash imply genetic or geographical affinity to the colder north? It certainly does not. The difference between the two cases finds elucidation, I think, in the phenology of the Fagales. In these the times of flowering seem at first to obey rules opposed to such as might be anticipated. It is a general rule that in one and the same species flowering is hastened in the season by a warmer climate, so that *Robinia Pseudacacia* in southern Italy produces blossom before acquiring foliage, and *Daphne Mezereum* on high mountains or far north postpones its flowering until leaves are produced. Yet the Betulaceæ, which extend farther north and are more characteristic of cold-temperate regions than the Fagaceæ, generally flower earlier in the season than do the oaks and chestnuts, many of whose species are sub-tropical or even tropical.

Here it would appear at first thought that the direction of evolution has been opposed to that in which the climate acts on the individual. Closer consideration, however, shows that such is not the case. In the Fagaceæ the inflorescences arise and open on the current year's shoot; in the Betulaceæ they likewise usually arise on the current year's shoot, though they do not open their flowers until the following vegetative season. Thus in *Castanea* and *Quercus*, allied to subtropical or tropical genera and species, and *Fagus*, whose distribution is more temperate, the inflorescences open in the year of their origin; whereas in the Betulaceæ, whose representatives extend farther north into arctic regions, but scarcely reach the tropical climate, the opening of the inflorescences is nearly always postponed until the year following their inception. That this explanation of the normal situation of the betulaceous inflores-cences on the one-year-old twigs is the correct one is confirmed by the behaviour of certain species of *Alnus*. The extreme northern American species, *A. sitchensis*, produces the inflores-

cence-buds in the normal manner so that these are visible in winter, but it does not adopt the normal habit of opening them before the leaves appear: in this species, blossoming is further postponed until the leaves unfold at any time between spring and midsummer. At the opposite extreme are three other species, namely, another American alder, *A. maritima*, which is more southern as it grows in Southern Delaware, Maryland and the Indian Territory; also two East Indian species, flourishing in a sub-tropical or tropical climate, *A. nepalensis* (descending to 3,000 feet on the Himalayas, and 6,000 feet in Upper Burma) and *A. nitida* (descending from the Himalayas on to the plains). In these three species, blossoming is hastened in comparison with other species of *Alnus*, for the inflorescences open in autumn as branches of the current year's shoot. The fact that *Alnus* is the betulaceous genus including species which show the fagaceous periodicity is of interest as supporting the view that the Betulaceæ are reduced Fagales living in a climate colder than the ancestral one, for *Alnus* is at the same time the genus having the most complete flowers as regards perianth, as well as undivided stamens, and thus showing the nearest approach to the typical fagaceous flowers of *Quercus* and *Castanea*.

The explanation of the early flowering of the Common ash and of the Elms and of the insertion of their inflorescences on the previous year's twig, is probably the same as that suggested here for the Betulaceæ, inasmuch as the near relations of both genera are most numerous in warmer countries.

Rhythmic change of form is well illustrated by the successional change in the dimensions of the leaves and internodes of a shoot produced during one vegetative season. The main axes of herbs and the long-shoots of trees representing one season's growth in length in cold-temperate regions generally show a gradual increase followed by a sudden or gradual decrease in the dimensions of the successive leaves and internodes. Branches of herbs display, according to position of insertion, either a similar rise and fall in length of internode, or a steady fall, or a rhythm transitional between these two. It might be thought that in cold-temperate plants the longer internodes owe their greater length to the higher temperature of summer compared with those of spring and autumn; but this suggestion in relation to both trees and herbs is disproved by the fact that growth in length is often concluded before

the temperature has reached its maximum. The inherent nature of this type of rhythm is demonstrated by the following facts:

1. These features present themselves in various land-plants developing at different times, growing in full sunlight or in shade, as well as in submerged plants; they are seen not only in cold-temperate trees but also, as Raciborski showed, in trees growing in perennially moist tropical forests, where there is neither a dry nor a cold season to interrupt vegetative activity.

2. On different individuals of the same species, and on different shoots of the same individual, both in cold-temperate and in moist tropical regions, the various shoots may entirely disagree as to their phase of development. In fact in the latter regions, as Schimper pointed out, a single tree may at one and the same moment have branches showing respectively resting buds, actively sprouting buds, flowers and fruits, thus recalling the conditions during winter, spring, summer and autumn in cold-temperate regions.

The greater length of the intermediate internodes is not due to the production of larger leaves on the same axis, for Miss Tinne Tammes has shown that removal of leaves from the stem does not destroy the periodic procession of internodes of differing lengths. It may also be noted that the changes of dimensions in leaves and internodes of a shoot, often at least, do not synchronise in phase.

Clearer insight into the nature of the longitudinal symmetry resulting from this periodicity of the internode-lengths may be gained by the construction of curves in which successive ordinates represent the lengths of the successive internodes. The resultant curve—the internode-curve—is often more or less characteristic of the species and therefore of diagnostic value, though it is subject to modification by external factors. The internode-curves of the axes of some herbaceous species are not more or less smooth and regular but are marked by zig-zags. In the investigated cases I have shown that these deviations, though to some extent due to variations in external factors, are largely inherent and due to longitudinal displacement of some of the leaves, as for instance when the phyllotaxis has changed from the cyclic to acyclic type. Very little work has been done on this subject; apparently irregularity in the

internode-curve is especially encountered in species, genera and families whose phyllotaxis varies from cyclic to acyclic.

It is a familiar fact that various species of herbs differ in their times of most active growth in length. Less generally known and often subject to misconception are the facts concerning the growth in length of trees. The statement may at once be made that this is governed by an innate periodicity and that the period of maximum growth in length coincides neither with maximum temperature of soil or air, nor with the maximum supply of humidity, nor with any combination of these. From among the series of papers dealing with this question those of Dr. D. Christison may be considered first, not only because of their extensive nature but because Christison simultaneously investigated in the same individual tree growth both in thickness and in length of stem. He showed that the periods of elongation and thickening do not synchronise either in duration or in phase. His observations in 1888 and 1889 on conifers were made on young specimens (ten to twenty years old) of *Abies lowiana* and *A. grandis*, *Pseudotsuga Douglasii*, *Pinus Pinaster* and *Araucaria imbricata*. Christison's results as tabulated below were obtained by taking the mean of the results obtained from all the coniferous specimens examined.

PERCENTAGE OF ANNUAL GROWTH TAKING PLACE IN THE BIMENSUAL PERIODS

Year.	Height of Top-shoot.			Girth of Trunk.		
	April-May.	June-July.	Aug.-Sept.	April-May.	June-July.	Aug.-Sept.
1888	6	78	16	35	35	30
1889	17	77	6	29	35	36

The results obtained in the two years agree in the main, and show that nearly 80 per cent. of the height growth took place in June and July, whereas growth in thickness is rather equably distributed over the season when measured according to bimensual periods. But the different species and even individual trees of one species differ in their periodicity of growth in length, and very different shares in growth are often taken by the two months of the bimensual periods. I have therefore roughly calculated the percentage share of elongation in the different (approximate) months for two of the species as measured by Christison.

Abies lowiana

	April 16–30.	May.	June.	July.	August.	September.
Specimen 98 in 1888.	0	3	11	**53**	33	0
„ 8 „ „ .	0	0	30	**53**	18	0
„ 8 „ 1889.	0	0	31	**61**	6	0
Pinus Pinaster						
Specimen 26 in 1888.	0	11	**48**	31	9	0
„ 26 „ 1889.	0	18	**58**	26	0	0

Thus in main features the behaviour of each species remained constant, *Pinus Pinaster* being earlier in its growth and attaining a maximum in June, *Abies lowiana* showing a maximum in July.

In regard to dicotylous trees Christison observed that the buds always commenced to swell and, except in the case of *Ulmus*, to open before growth in thickness began *in the trunk*. It is necessary, however, to avoid forming the conclusion from these facts that there is a difference in the time of awakened activity of these two forms of growth, inasmuch as the cambium becomes active in twigs before it awakens in the trunk. Christison gave no particulars as to the season of elongation of dicotylous tree-stems, but we know that a number of these are capped by resting buds or terminal scars relatively early in the vegetative season (for instance, I have found even the lime regularly in this condition at the middle of June).

In regard to growth in length of stems and the lengths of the successive internodes, a number of interesting problems concerning the effect of the innate rhythm on the response to external influences appear not to have been investigated. For instance, if a growing stem destined to attain a certain length under normal circumstances be subjected at a certain phase to an inhibiting influence (say lack of water), will it, if once more provided with favourable conditions, attain its approximate full length by change in length or number of the succeeding internodes? What are the relative effects on growth in length of the same modifying influences applied at different phases? In general what influence has the correlation between the successive internodes on the nature of the response to external change?

It has been shown that the seasons of growth in length and of thickness of a stem do not synchronise in period; the same is true of the seasons of elongation of the leaves and of the

stem bearing them, at least in conifers. Meissner proved this in the case of *Pinus sylvestris* and *Picea excelsa*, and from his results obtained in reference to the former I have roughly calculated the percentage share of the whole growth in length of the year's stem and the needles supplied in months of the growing season. The statistics are given in the succeeding table, which also contains the corresponding particulars as to growth in thickness at approximately the same time, as measured by Mischke, whose work is described later in this article. (The dates in brackets are Mischke's, the others are Meissner's.)

PERCENTAGE OF GROWTH OF *PINUS SYLVESTRIS* DURING SUCCESSIVE PORTIONS OF THE GROWING SEASON

	Up to May 13 (13).	May 13 (13) to 29 (27).	May 29 (27) to June 28 (27).	June 28 (27) to July 27 (24).	July 28 (24) to Aug. 25 (9).
Elongation of Stem .	15·5	40·2	42·4	2·1	0
„ „ Leaf .	0	10·7	49·5	33	6·8
Increase in thickness of stem (as judged by number of tracheids)	18 2	18·2	50	0	13·6

In two of the shoots of the Scots Pine examined by Meissner the stem had attained by the end of June 98 and 99 per cent. of its full length, whereas the leaves had attained only 60 per cent. In a third shoot the percentages of the full growth for the stem were 92 before the middle of June, almost 99 at the end of June, while the corresponding percentages for the leaves were 34 and 60. Thus in *Pinus sylvestris* the leaves lag behind the stem as regards growth in length. Meissner observes that the reverse is the case in *Picea* and *Abies*, whose leaves are more forward in development than the stem.

Questions concerning the periodicity of growth in thickness of woody stems and roots have been investigated by methods of two different types. The microscopical method of observing the behaviour of the cambium to determine the dates of commencement and conclusion of its activity was used by von Mohl, T. and R. Hartig, Russow, Mer, Gulbe and Strasburger; it was utilised by R. Hartig to determine the fractional parts of the annual ring produced during the successive portions of the growing season. Mischke used the very delicate method of counting the number of tracheids in the same annual ring

of conifers at repeated intervals during the period of growth. The macroscopic method was adopted by Sir R. Christison, and subsequently by Dr. D. Christison, who observed the rate of increase by means of a tape measure (made of steel in the case of the latter's observations). The macroscopic method measures the increments due to a complex of phenomena—namely, the algebraic sum of (1) additions to the bark and wood caused by the phellogen and cambium; (2) expansion and contraction due to increased or decreased supplies of water and heat; (3) decrease due to peeling of dead bark; (4) change due to cracking of the bark; (5) expansion and contraction of the steel tape due to change of temperature. Selection of appropriate trees eliminated the difficulties concerning the dead bark. It was evident that contraction with loss of water or fall of temperature could take place, as sometimes a slightly decreased girth was registered by the tape after the lapse of a week. Yet, that Dr. Christison's results may be accepted as trustworthy is demonstrated by the consistent character of his results and their general agreement with those of others who used the microscopical method.

The date of commencement of activity of the cambium in one and the same species varies with the part of the tree in one individual and in different individuals according to site and climate. In central Germany it begins to be active usually in the second half of April or the first half of May. At Edinburgh at five feet from the ground in the main trunk Christison found that in 1890 the dates at which the different species commenced and ceased to thicken varied from April 6 to May 17 and from August 23 to October 11 respectively. Strasburger states that the production of phloem is prolonged later in the season than that of xylem: this would account for the fact that compared with the results obtained by continental observers, who fixed their attention on the production of the wood, Christison's statistics show that the process of thickening is prolonged later into autumn.

The exact time at which the several parts of the annual ring are produced has been investigated by several botanists, who have demonstrated, *inter alia*, that the term "autumn wood" conveys a false impression. Mer, for instance, found that in the oak the transition from "spring" to "autumn" wood is effected about the middle of June and that in conifers the

"autumn" wood is generally initiated about the middle of August. Hartig found that in beeches, fifty to 150 years old, the following fractions of the annual ring were produced: one-third up to the middle of June, half up to the middle of July, three-quarters up to the end of July and that the ring was complete at the middle of August. Mischke obtained interesting results by counting at intervals of several days the number of tracheids constituting the growing annual ring of *Pinus sylvestris* and *Picea excelsa*. Statistics in reference to the former are given in the preceding table: in this specimen the production of new tracheids commenced at the beginning of May and ended about August 25. Three facts worthy of note were demonstrated in connection with this slow-growing specimen of *Pinus*: (1) There was a rise and fall in the rate at which the tracheids were produced. (2) This general rise and fall was interrupted by a complete stoppage in the production and maturation of the tracheids between June 27 and July 24. (3) The outermost part of the annual ring was ultimately constituted of several (three) ranks of initial cells (that is, tracheids remaining in a juvenile condition) destined to be converted in the following year into "spring" tracheids. The same three features were noted in the normally growing *Picea excelsa*: the cessation in the production of tracheids lasted between June 27 and July 11 but in this case some of the young tracheids already present thickened their walls during the passive period. Mischke discovered, too, that although no new tracheids were produced after August 25 the differentiation and lignification of the outer ones continued during about a month longer. Mischke attributed the stoppage in the middle of summer to the abnormally small rainfall in the year of observation (1888) during April, May and June. But this explanation cannot be accepted in the face of the evidence supplied by D. Christison, who showed that the general rise and fall in the rate of thickening of conifers was interrupted by a marked fall in July in each of the three years of observation: in two of these years the summer was unusually cold and wet, whilst the other summer was exceptionally warm and dry. From Dr. Christison's statistics I select as examples those dealing with two species measured in 1888: as his figures are recorded for unequal numbers of days, I have calculated from them the mean diurnal increment during these unequal successive periods: the results are given in inches in the succeeding table:

MEAN DIURNAL INCREMENTS OF GROWTH IN GIRTH AND HEIGHT DURING THE NAMED SUCCESSIVE PERIODS

	April 16 to 30.	May 1 to 14.	May 15 to 31.	June 1 to 16.	June 17 to July 4.	July 5 to 16.	July 17 to Aug. 1.	Aug. 2 to 16.	Aug. 17 to Sept. 1.	Sept. 2 to 31.
Abies lowiana:										
Height . .	0	0	0	·0625	·194	·375	·291	·1	·075	0
Girth . .	·003	·014	·018	·0125	·005	·0083	·0031	·006	·0125	·006
Pinus Pinaster:										
Height . .	0	·0373	·118	·156	·4	·27	·22	·13	0	0
Girth . .	·003	·007	·012	·00625	·011	·0083	·0062	0	·0062	0

In reference to the mysterious temporary stoppage of growth in thickness Dr. Christison concludes that "the retardation in girth-increase in conifers is synchronous with rapid growth of the new top and therefore may be due to the incapacity on the part of the tree to carry on the two growths in full measure at the same time." But an inspection of the figures given in the table above shows that though they harmonise well with Christison's hypothesis in the case of *Abies*, this is not true in the case of *Pinus*: yet in both these species the second maximum of rate of growth in thickness occurring in late summer synchronises with great reduction or stoppage in the rate of growth in height. The cause of the halt in thickening then is unknown. Possibly it is due to temporary shortage of water or of food-material that are being diverted to growth in length of the stem and leaves and to transpiration; available statistics give no support to this suggestion, in particular von Höhnel's figures concerning the monthly rate of transpiration in conifers betray no signs that transpiration is a contributory cause. We do not know whether this growth in thickness is checked simultaneously throughout the tree or if it is synchronous with a sudden increase in rate of thickening of other parts of the tree (the root, for instance). Possibly it may be a genuinely innate pause, such as we might anticipate could characterise a species whose ancestral home lay in a region where there was a rest in the middle of the hot season associated with drought, and where the climate so far approximated to that now prevailing in the regions characterised by sclerophyllous vegetation. From this point of view the investigation of living sclerophyllous types would be of interest. This revived activity of the cambium late in the season of growth is by no means

without analogy, as conifers and perhaps some American species of evergreen oaks produce "double" annual rings, while certain trees, the oak, for instance, emit a second crop of leafy shoots—the lammas shoots.

The periodicity revealed in the production of the annual ring is known to be innate. Annual rings are most marked in deciduous species and sometimes distinct in evergreen species (as in many conifers and some oaks); where they are unrecognisable the species is evergreen. So far as is known the seasons of production of the annual ring and the new leaves, both in deciduous dicotyledons and evergreen conifers, fall at the same time of the year, though the cycles of these two forms of morphogenous activity appear to coincide neither in exact times of initiation, conclusion nor phase. The close association of the two processes is exemplified by an interesting observation made by J. Carruthers or Holtermann on a chocolate-tree which was between $7\frac{1}{3}$ and $7\frac{1}{2}$ years old. In each year of its existence the treelet lost its leaves and acquired new ones three times; and when cut down the main stem showed at its base twenty-two "annual" rings. Yet in cold-temperate regions the production of a second crop of leaves in the one vegetative season, either on the lammas shoots or after complete defoliation, does not necessarily involve the production of a second "annual" ring, though in some cases such a doubling of the annual ring ensues. Hence it must be of interest to learn what relation subsists in the tropics between the seasons of leaf-production and stem-thickening, especially in connection with evergreen trees growing in the perennially moist forests. When one of these latter trees has branches or large boughs that differ as regards the time of the year during which they produce new leaves, two possibilities present themselves. Either there may be a definite time (which may extend throughout the whole) of the year during which the trunk and branches thicken; or when each branch produces new leaves its cambium may become active, so that the cambium in different branches would be active at different times of the year. The consequence of the latter mode of thickening might be that the activity of the cambium would then extend down from the branch to the parent axis, perhaps affecting only that side on which the branch is inserted. The result of such a mode of growth would be the deposit of new wood in the form not of rings but of arcs, and in the cross section of the trunk or large

bough there would be arc-like zones of growth running into one another, just as do arcs of cork in thick bark. Investigation may show that this is the explanation of the peculiar structure of the wood of many tropical species.

Finally, it may be pointed out that the growth in height and in thickness of the tree as a whole follows a waxing and waning rhythm, so that the successive annual rings traced from the centre at first widen and subsequently narrow until they become approximately constant in thickness.

The advantages accruing to the plant through certain forms of periodicity are clear, as in the cases of resting spores and seeds; the annual shedding of leaves or death of epigeous parts; the early flowering of some trees before foliage obstructs the view to insects or opposes a barrier to the wind; the earlier unfolding of foliage of some plants before these are overshadowed by rivals; the differences in the times of leafing, flowering and fruiting in various species, which thus avoid contemporaneous competition for the various chemical elements; the rapid upright growth of young light-demanding trees contrasting with the slow, often more spreading, growth of young shade-enduring species. But in other cases the benefit to the plant of certain forms of periodicity is unknown or possibly non-existent, in the latter case being due perhaps to inevitable correlation or to the bonds imposed by ancestry.

THE PART PLAYED BY SIR CHARLES BELL IN THE DISCOVERY OF THE FUNCTIONS OF MOTOR AND SENSORY NERVES (1822)

By A. D. WALLER, M.D., F.R.S.

In time to come, the discovery of motor and sensory nerves during the first quarter of the nineteenth century will rank with that of the circulation of the blood in the first quarter of the seventeenth century. "Harvey, 1628," will then as now be the human time-mark of the earlier discovery. What will be that of the more recent and equally far-reaching principle that the nervous impulses controlling our conscious and unconscious life flow by separate channels from world to brain and from brain to world? Who was the actual discoverer of what Galen imagined two thousand years ago?

To-day, in 1911, "Charles Bell, 1811," are the name and date that are in this country associated with that discovery.

The first and principal object of this study is to prove that these are the wrong name and the wrong date and that the discovery belongs exclusively to another name and date—namely to "Magendie, 1822."

Twenty years ago, in preparing *An Introduction to Human Physiology*, I summarised my conclusion in the following terms after careful study of the original documents:

"All these facts, inclusive of those dependent on the existence of recurrent sensory fibres, were discovered by Magendie (1822). Charles Bell (1811), to whom they are commonly ascribed, observed motion on excitation of the anterior roots of a recently killed animal and inferred that the anterior or 'cerebral' roots are motor and sensory and that the posterior or 'cerebellar' roots serve to govern vital actions. A. Walker (1809) made the unfortunate guess that the anterior roots were sensory and the posterior motor."—*Introduction to Human Physiology*, 1st edition, 1891, p. 475.

A few months ago, in the *Lancet* of February 18, 1911, Prof. Keith—Bell's successor in the chair of Anatomy at the

College of Surgeons—delivered an address at the Middlesex Hospital—Bell's hospital—very properly laudatory of Bell's career but very improperly promulgating the misrepresentations of Bell's "discoveries" that were elaborated from 1823 to 1839 by Bell himself and by his two devoted brothers-in-law, John and Alexander Shaw.

The family compact according to which Bell was greater than Harvey is intelligible enough as a family compact but the marvel, in this case, has been that Bell's pretensions should have been admitted not only in this country but also to some extent in Germany and in France, where it has been said that though Bell missed the actual discovery yet he came very close to making it. As we shall presently see, Bell's claim to the discovery is analogous with that of a marksman who having made two shots equally wide to right and left of the target, should claim that two such shots are the equivalent of a double bull's eye. Bell failed to discover the functions of the nerve roots in 1811 and failed to discover the functions of the facial nerves in 1821; upon these two equally complete failures is based his claim to both discoveries.

Magendie and Bell, 1822–4

In 1822, Magendie, aged thirty-nine, at the zenith of his powers, the leading physiologist of France, published the following paper in the second volume of the *Journal de Physiologie*,[1] founded by himself in the previous year:

"Depuis longtemps je désirais faire une expérience dans laquelle je couperais sur un animal, les racines postérieures des nerfs qui naissent de la moelle épinière. Je l'avais tentée bien des fois, sans pouvoir y réussir, à cause de la difficulté d'ouvrir le canal vertébral sans léser la moelle, et par suite sans faire périr ou tout au moins sans blesser grièvement l'animal. Le mois dernier, on apporta dans mon laboratoire, une portée de huit petits chiens, âgés de six semaines; ces animaux me parurent très propres à tenter de nouveau d'ouvrir le canal vertébral. En effet, je pus à l'aide d'un scalpel bien tranchant, et pour ainsi dire d'un seul coup, mettre à nu la moitié postérieure de la moelle épinière entourée de ses enveloppes. Il ne me restait pour avoir cet organe presqu'à nu, que de couper la dure-mère qui l'entoure; c'est ce que je fis avec facilité; j'eus alors sous les

[1] "Expériences sur les fonctions des racines des nerfs rachidiens," tome . 1822, p. 276.

yeux les racines postérieures des paires lombaires et sacrées, et en les soulevant successivement avec les lames de petits ciseaux, je pus les couper d'un côté, la moelle restant intacte. J'ignorais quel serait le résultat de cette tentative; je réunis la plaie par une suture à la peau, et j'observai l'animal; je crus d'abord le membre correspondant aux nerfs coupés, entièrement paralysé; il était insensible aux piqûres et aux pressions les plus fortes, il me paraissait aussi immobile; mais bientôt, à ma grande surprise, je le vis se mouvoir d'une manière très apparente, bien que la sensibilité y fût toujours tout-à-fait éteinte. Une seconde, une troisième expérience, me donnèrent exactement le même résultat; je commençai à regarder comme probable que les racines postérieures des nerfs rachidiens pourraient bien avoir des fonctions différentes des racines antérieures, et qu'elles étaient plus particulièrement destinées à la sensibilité.

"Il se présentait naturellement à l'esprit de couper les racines antérieures, en laissant intactes les postérieures; mais une semblable entreprise était plus facile à concevoir qu'à exécuter; comment mettre à découvert la partie antérieure de la moelle, sans intéresser les racines postérieures? j'avoue que la chose me parut d'abord impossible; cependant je ne cessai d'y rêver pendant deux jours, et enfin je me décidai à essayer de passer devant les racines postérieures, une espèce de couteau à cataracte, dont la lame, très étroite, permettrait de pouvoir couper les racines, en les pressant avec le tranchant de l'instrument, sur la face postérieure du corps des vertèbres; mais je fus obligé de renoncer à cette manœuvre, à cause des grosses veines que contient le canal de ce côté, et que j'ouvrais à chaque mouvement en avant. En faisant ces essais, je m'aperçus qu'en tirant sur la dure-mère vertébrale, on pouvait entrevoir les racines antérieures réunies en faisceaux, au moment où elles vont percer cette membrane. Il ne m'en fallut pas davantage, et en quelques instans, j'eus coupé toutes les paires que je voulais diviser. Comme dans les expériences précédents, je ne fis la section que d'un seul côté afin d'avoir un terme de comparaison. On conçoit avec quelle curiosité je suivis les effets de cette section: ils ne furent point douteux, le membre était complètement immobile et flasque, tandis qu'il conservait une sensibilité non équivoque. Enfin, pour ne rien négliger, j'ai coupé à la fois les racines antérieures et les postérieures; il y a eu perte absolue de sentiment et de mouvement.

"J'ai répété et varié ces expériences sur plusieurs espèces d'animaux: les résultats que je viens d'énoncer se sont confirmés de la manière la plus complète, soit pour les membres antérieurs, soit pour les postérieurs. Je poursuis ces recherches et j'en donnerai un récit plus détaillé dans le prochain numéro; il me suffit de pouvoir avancer aujourd'hui comme positif, que les racines antérieures et les postérieures des nerfs qui naissent à

la moelle épinière, ont des fonctions différentes, que les postérieures paraissent plus particulièrement destinées à la sensibilité, tandis que les antérieures semblent plus spécialement liées avec le mouvement."

This short paper of Magendie's is a model of description; everything that had to be said is clearly said; there is no unnecessary phrase or word, the narrative being plainly that of the man who wanted to know, who went straight to the point and who tells clearly what he has seen.

Any one acquainted with the history of scientific discovery will admit readily that here is a case of a discovery of first magnitude stated by its author in language that cannot be improved by paraphrase or elision. There is no other case that I know of in Physiology in which a man's chief contribution can be thus presented "en bloc" in the discoverer's own words; we are as a rule obliged to paraphrase or summarise and can at best weave into our word-picture some few phrases from the original story told by the discoverer, unless we give his story in full; and then how much must we not reprint that is obsolete and redundant. There is nothing either obsolete or redundant in Magendie's paper of 1822.

In 1823, Charles Bell, aged forty-nine, surgeon to the Middlesex Hospital and lecturer on Anatomy at the Great Windmill Street School, hearing of these experiments, calls them *his* experiments and evidently believes that they are his. He has recently (1821, 1822, 1823) published some observations on the facial nerves and ten years previously (1811) he printed a pamphlet about nerves. He naturally feels, therefore, a kind of vested interest in nerves; and since John Shaw told Magendie about Bell's views and methods two years before (1821) and since Magendie expressed himself as delighted and astonished, Magendie must have borrowed from him.

Now so far all this is very human and quite pardonable. A man who has thought round and about something is apt to deceive himself when another man puts his finger on the spot and picks out the plum; he may be forgiven for exclaiming, "But that's my plum; I found it long ago and showed it to that fellow who says he found it." A claim of this sort may be good or bad; the claimant may be entitled to credit—all or

some or none—according to the evidence that he may have to offer; but he is certainly entitled to nothing but discredit if he set to work to improve a poor claim by tampering with his own evidence; this, as we shall see, is precisely what Bell did in 1824, when he republished his papers of the three preceding years. Let us examine his claim.

Bell and the Nerve Roots.—Bell's claim to the discovery that motor impulses pass by the anterior, sensory impulses by the posterior spinal roots is based (1) upon a small 12mo booklet of 32 pages entitled *Idea of a New Anatony of the Brain*, printed (not published) in 1811 for the observations of the author's friends[1]; (2) upon the first of a series of six papers published from 1821 to 1829 in the *Philosophical Transactions of the Royal Society.*

The booklet of 1811, although short enough, is not worth reprinting here in full; the passages upon which Bell himself laid stress in the preface of his *Nervous System* of 1830 contain all that can be deemed as having any bearing upon the question.

Bell, 1811, *Idea of a New Anatomy of the Brain*, p. 21:

"I took this view of the subject. The *medulla spinalis* has a central division, and also a distinction into anterior and posterior fasciculi, corresponding with the anterior and posterior portions of the brain. Further, we can trace down the crura of the *cerebrum* into the anterior fasciculus of the spinal marrow, and the crura of the *cerebellum* into the posterior fasciculus. I thought that here I might have an opportunity of touching the *cerebellum*, as it were, through the posterior portion of the spinal marrow and the *cerebrum* by the anterior portion. To this end I made experiments which, though they were not conclusive, encouraged me in the view I had taken. I found that injury done to the anterior portion of the spinal marrow convulsed the animal more certainly than injury done to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

"Next, considering that the spinal nerves have a double root, and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain,

[1] *Idea of a New Anatony of the Brain*, submitted for the observations of his friends by Charles Bell, F.R.S.E. (small 8vo, printed by Strahan & Preston, printers, London; without date, but on collateral evidence the date 1811 is accepted). Sir Charles Bell's copy of this rare pamphlet was presented to the Royal Society by Professor Jeffrey Bell in 1893. A reprint is given in Humphry and Turner's *Journal of Anatomy*, 1869 (vol. iii. p. 147).

I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord and held together by the same sheath.

"On laying bare the roots of the spinal nerves, I found that I could cut across the posterior portion of the spinal marrow without convulsing the muscles of the back; but on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.

"Such were my reasons for concluding that the *cerebrum* and the *cerebellum* were parts distinct in function, and that every nerve possessing a double function obtained that by having a double root. I now saw the meaning of the double connection of the nerves with the spinal marrow; and also the cause of that seeming intricacy in the connections of nerves throughout their course, which were not double at their origins.

"The spinal nerves being double, and having their roots in the spinal marrow, of which a portion comes from the *cerebrum*, they convey the attributes of both grand divisions of the brain to every part; and therefore the distribution of such nerves is simple, one nerve supplying its destined part. But the nerves which come directly from the brain come from parts of the brain which vary in operation; and in order to bestow different qualities on the parts to which the nerves are distributed, two or more nerves must be united in their course or at their final destination. Hence it is that the 1st nerve must have branches of the 5th united with it: hence the *portio dura* of the 7th pervades everywhere the bones of the cranium to unite with the extended branches of the 5th: hence the union of the 3rd and 5th in the orbit: hence the 9th and 5th are both sent to the tongue: hence it is, in short, that no part is sufficiently supplied by one single nerve, unless that nerve be a nerve of the spinal marrow, and have a double root, a connection (however remotely) with both the *cerebrum* and *cerebellum*."

P. 26:

"The *cerebellum* when compared with the *cerebrum* is simple in its form. It has no internal tubercles or masses of cineritious cortex, and forms the *crus*; and the *crus* runs into union with the same process from the *cerebrum*; and they together form the *medulla spinalis*, and are continued down into the spinal marrow: and these cura or processes afford double origin to the double nerves of the spine. The nerves proceeding from the *Crus Cerebelli* go everywhere (in seeming union with those from the *Crus Cerebri*); they unite the body together, and control the actions of the bodily frame; and especially

govern the operation of the viscera necessary to the continuance of life."

P. 27:

"The *cerebrum* I consider as the grand organ by which the mind is united to the body. Into it all the nerves from the external organs of the senses enter; and from it all the nerves which are the agents of the will pass out."

P. 36:

"The secret operations of the bodily frame, and the connections which unite the parts of the body into a system, are through the *cerebellum* and nerves proceeding from it."

There are several evident conclusions to be drawn from any fair consideration of these passages—firstly that Bell does *not* say that anterior roots are motor and posterior sensory; secondly that Bell *does* say that the cerebral or anterior root is motor and sensory, the cerebellar or posterior root insensitive; thirdly that the so-called experiment of dividing the roots as described by Bell did not entitle him to say anything more than he did. And what he said was no advance upon the knowledge of his time. It is ludicrous to appeal to such speculations as containing an anticipation of Magendie's lucid and precise account quoted above.

There is little enough in these passages and there is nothing at all of any significance in the rest of the pamphlet, which even as a purely speculative essay is very poor stuff—far less precise indeed than the ordinary speculative anatomy of that time. It is indeed nothing more than a very inferior echo of the anatomical reasonings published by Alexander Walker in 1809.

Walker then said (*New Anatomy and Physiology of the Brain, etc., Archives of Universal Science*, vol. iii. July 1809, p. 172):

". . . wherever a part, having both sensation and motion, is supplied from one nervous trunk it envelopes both a nerve of sensation and one of volition The only apparent difference between the nerves of sensation and those of volition is that their motions take place in different directions. The latter therefore may be said to resemble the arteries, the former the veins."

In a later publication—*Thomson's Annals of Philosophy*, July 1815—he says:

"It appears then that there is a species of circulation in the nervous system, of which I have sketched the general course, as curious and admirable as that which exists in the vascular (the centre of the one being the heart, of the other the head); and that there is scarcely any point of the body which this circle does not involve and rest on, since from almost every point ascends impression to the cerebrum by a nerve of sensation, the anterior nervous roots, and the anterior columns of the spinal marrow, and to each returns expression from the *cerebellum* by the posterior columns, the posterior nerve-roots and the nerves of volition."

Bell preferred his claim to Magendie's discovery at once, in 1822, by proxy, through John Shaw, the same brother-in-law who had gone over to Paris in 1821 to show Bell's experiments on the facial nerves. Magendie at once printed John Shaw's reclamation in his *Journal de Physiologie* (vol. iii. 1822, p. 270) and acknowledged it with a liberality of which Bell and his followers subsequently took advantage. Magendie, who at that time regarded Bell as a serious worker, accepted Bell's statement that he had cut the anterior and posterior roots as meaning what he, Magendie, meant when he talked about cutting anterior and posterior roots, *i.e.* section *in vivo*, not, as he subsequently found, section *post mortem*. And so he allows Bell the credit of having cut the spinal roots before he did and of having very nearly discovered their functions. This concession of Magendie's was more than Bell ever deserved; the experiment of 1811 to which Bell alludes was made not on the living but on the recently killed animal; he inferred from his results, not that the anterior root was motor and the posterior sensory but that the anterior root was the cerebral (*i.e.* motor and sensory) channel and the posterior root the cerebellar (*i.e.* secret and vital) channel. The younger Shaw—Alexander—subsequently made capital out of this admission of Magendie's[1]; even at the present day its consequences persist in the form of the loose statement that

[1] A. Shaw (*Narrative of the Discoveries of Sir Charles Bell in the Nervous System*: London, 1839) says at p. 156: "In the succeeding number of his *Journal of Physiology* (October 1822), or two months after writing his first paper, M. Magendie admitted that the experiments on the roots of the spinal nerves, which he had claimed as original, had been performed many years before by Sir Charles Bell."

"Bell first made the experiment in 1811 while Magendie confirmed and extended it ten years later," whereas the truth of the matter is that Magendie was the real and sole author both of the experiment and of its interpretation. The Bell faction of that day went indeed further than this. At the Cambridge meeting of the British Association in 1833 the statement was made and accepted unchallenged that "the honour of this discovery, doubtless the most important accession to physiological knowledge since the time of Harvey, belongs exclusively to Sir Charles Bell." More recently still we have Sir Charles Bell's successor at the College of Surgeons—Prof. Keith—making the staggering assertion that "nothing can be clearer than Bell's title to the discovery that the anterior root is motor and the posterior sensory," *because* Bell says that "the anterior root is sensitive and the posterior root insensitive.[1] This surely is the *ne plus ultra* of the Bell fable; it is time to pull up and to realise the meaning of cold print.

From now onwards until the end of his life Bell harps upon Magendie's discovery as his; the 5th nerve as "the spinal nerve of the head" soon becomes his principal war-horse. The extraordinary feature of Bell's scientific career is that from 1824 to 1844, in the numerous papers on the nervous system that he published during that period, he never made any experiment nor stated any new fact. Every one of these papers, whatever its subject, contains, however, a repetition of his first false claim to Magendie's discovery in one form or another—generally as an allusion to something taken for granted. And where it is not Bell who speaks, it is Alexander Shaw, either in his own name or as "a pupil."

It was during 1823 that Bell prepared his case. In that year he published two papers on the Eye in the *Philosophical Transactions*, the first part on March 20, the second on June 19. In the second, at pp. 306–7, he attacks Magendie, condemning experiments and appealing to his pupils to keep to the examples of our own great countrymen. This is what he writes in the *Philosophical Transactions*, p. 302:

'Anatomy is already looked upon with prejudice by the thoughtless and ignorant: let not its professors unnecessarily incur the censures of the humane. Experiments have never been the means of discovery; and a survey of what has been

[1] *The Lancet*, March 18, 1911, p. 765.

attempted of late years in physiology will prove that the opening of living animals has done more to perpetuate error than to confirm the just views taken from the study of anatomy and natural motions."

And at p. 307:

"Surely it is time that the schools of this kingdom should be distinguished from those of France. Let physiologists of that country borrow from us, and follow up our opinions by experiments (see the experiments of M. Magendie on the distinctions in the roots of the spinal nerves); but let us continue to build that structure which has been commenced in the labours of the *Munros* and *Hunters*."

The claim thus started in 1823 was followed up by the publication in the following year of *An Exposition of the Nervous System*, of which the subsequent editions (1830, 1836 and 1844) have served as the usual sources of reference and quotation by subsequent writers. It is important therefore to examine this 1824 edition with some little care and to compare the republications given in it with the original publications of 1821.

Bell's *Exposition* of 1824 consists of an introduction of 64 pages followed by a republication of his first four papers in the *Phil. Trans. of the R.S.* The Introduction substantially incorporates all the statements published by Magendie during the two previous years, without however once naming him. But that Magendie was prominent in Bell's mind at the time of writing is obvious enough to any one acquainted with the publications of the French physiologist; it is indeed sufficiently indicated by his allusion to "experiments" on p. 2:

"In France, where an attempt has been made to deprive me of the originality of these discoveries, experiments without number and without mercy have been made upon living animals; not under the direction of anatomical knowledge or the guidance of just deduction, but conducted with cruelty and indifference, in hope to catch at some of the accidental facts of a system which, it is evident, the experimenters did not fully comprehend."

This introduction to Bell's *Exposition* of 1824 contains a correct account of the essential physiological facts. The anterior column and the anterior root are motor, the posterior column

and the posterior root are sensory. But this correct account is a consequence of Magendie's work of 1822.

If only this introduction had been written in 1821, we should unhesitatingly have accepted Charles Bell as the author of a great and far-reaching discovery. But it was written in 1824; it incorporates Magendie's discoveries of 1822 on the roots and of 1823 on the columns. Whilst he is not named, Magendie is clearly enough indicated. Bell's plagiarism at the expense of Magendie is glaring.

At pp. 40–41 of his *Exposition* of 1824 he says:

"I have now only to add, that these opinions and experiments have been followed up to the satisfaction of all Europe. It has been acknowledged that the anterior roots of the spinal nerves bestow the power of muscular motion; and the posterior roots sensibility. When the anterior roots of the nerves of the leg are cut in experiment, the animal loses all power over the leg, although the limb still continues sensible. But if, on the other hand, the posterior roots are cut, the power of motion continues, although the sensibility is destroyed. When the posterior column of the spinal marrow is irritated the animal evinces sensibility to pain; but no apparent effect is produced when the anterior column is touched."

We have seen above what Magendie had said of the roots in 1822. In the *Journal de Physiologie*, vol. iii. 1823, p. 153, he had expressed himself as follows about the columns:

"*Sur le siège du mouvement et du sentiment dans la Moelle Epinière.*—Les expériences que j'ai publiées sur les fonctions des racines des nerfs spinaux, et qui montrent que les antérieures sont destinées au mouvement, tandis que les postérieures appartiennent au sentiment, conduisaient naturellement à examiner si les côtés antérieurs et postérieurs de la moelle épinière ne seraient point en rapport de propriétés avec les filets nerveux. Le fait confirme ici la conjecture; si on met à nu la moelle dans un point quelconque de sa longueur, et si on la touche ou la pique doucement en arrière, sur les deux cordons placés entre les racines postérieures, l'animal donne des signes d'une exquise sensibilité; si, au contraire, on fait les mêmes tentatives sur lar partie antérieure, les indices de sensibilité sont à peine visibles."

Bell's drafting of this passage is characteristic both as regards its magniloquence and its indirectness. The phrase "It has been acknowledged" implies that the facts had been previously

announced by the writer, *i.e.* that the anterior roots are motor, the posterior sensory; and that the posterior columns are sensitive, the anterior insensitive. Can it be said on Bell's behalf that he does not claim the facts and the experiments as his? I think not. He does not name Magendie, although obviously he is making use of results by Magendie, transferring them to himself by implication, oblivious of what he has himself said a few pages before (29–31) about the different roots and different columns of the cord—*i.e.* that the anterior roots and columns when irritated give motion, whereas no motion follows irritation of the posterior roots.

Bell seems to have realised in later editions that the passage was too strong. In 1830 he tones the claim down a little by substituting the phrase "It has been confirmed" for "It has been acknowledged." And in the third edition (1836 and 1844), the concluding sentence about irritation of the columns was deleted.

Obviously Bell realised that such an experiment could not be advanced as his (Bell's) property. And now a footnote implying only that his "discovery" of anterior roots (motor) and of posterior roots (sensory) has been confirmed by Tiedemann and by Muller is substituted for the compromising incorporation of Magendie's results as to the columns.

The Introduction of 1824 concludes with the following sentence (p. 66):

"I will now lay before my reader the papers which I presented to the Royal Society on this subject, and in the order they are printed in the *Philosophical Transactions.*

And then follow the first four papers republished from the *Philosophical Transactions of the Royal Society*, without any warning of any kind that they are altered from their original form. These four papers and two others form a group of six papers, published in the *Philosophical Transactions of the Royal Society* from 1821 to 1829, which constitute the chief evidence upon which Bell's quality as a man of science is to be appreciated.

Bell's Six Papers, 1821 to 1829

Up to this point we have realised that there is no trace to be found of any discovery of the functions of the spinal roots in 1811

—Bell's *Idea of a New Anatomy of the Brain* met with no more attention than it deserved; his ambition "to make the town ring with it" was not crowned with success nor did it serve "to swell his name into importance." During the next ten years Bell made no further attempt to develop the *Idea*; in 1821, when he sent his first paper on the Nerves to the Royal Society, he does not allude to it.

Bell, now of middle age, makes his second bid for scientific fame and begins to send papers to the Royal Society. The period 1821 to 1829 is that of his greatest activity as a physiological investigator. At first he has the assistance of his brother-in-law John Shaw, later, *i.e.* from 1827, that of Alexander Shaw, who left Cambridge to take his brother's place. During this period Bell published the six papers in the *Philosophical Transactions of the Royal Society*, to which we alluded above, upon which alone his claim must stand or fall. And strictly speaking it is only to the first two of these papers, which precede Magendie's discovery of 1822, that appeal can be made in favour of Bell. Since, however, these six papers form a connected group of which the first and sixth contain the most crucial part of Bell's case—that relating to the fifth nerve—it is necessary to review or at least to enumerate them as a whole.

The six papers are as follows:

(1) 1821. *On the Nerves: giving an account of some experiments on their structure and functions, which lead to a new arrangement of the system.* By Charles Bell, Esq. Communicated by Sir Humphry Davy, Bart., P.R.S. —Read July 12, 1821.—*Philosophical Transactions of the Royal Society*, 1821, Part II. pp. 398–424.

(2) 1822. *Of the Nerves which associate the muscles of the chest in the actions of breathing, speaking and expression. Being a continuation of the paper on the Structure and Functions of the Nerves.* By Charles Bell, Esq. Communicated by Sir Humphry Davy, Bart., LL.D., P.R.S. —Read May 2, 1822.—*Phil. Trans. R.S.* 1822, Part II. pp. 284–312.

(3) 1823. *On the Motions of the Eye, in illustration of the uses of the Muscles and Nerves of the Orbit.* By Charles Bell, Esq. Communicated by Sir Humphry Davy, Bart., P.R.S.—Read March 20, 1823.—*Phil. Trans. R.S.* 1823, p. 166.

(4) 1823. *Second part of the paper on the Nerves of the Orbit*, by Charles Bell, Esq.—Read June 19, 1823.—*Phil. Trans. R.S.* 1823, p. 289.

(5) 1826. *On the nervous circle which connects the voluntary Muscles with the Brain.* By Charles Bell, Esq. Communicated by the President, January 25, 1826.—Read February 16, 1826.—*Phil. Trans. R.S.* 1826, Part II. p. 163.

(6) 1829. *On the Nerves of the Face; being a second paper on that Subject.* By Charles Bell, Esq. F.R.S.—Read May 28, 1829.—*Phil. Trans. R.S.* 1829, p. 317.

Besides these six papers, Bell published three general accounts of the Nervous System in 1824, 1830 and 1836, which are, in fact, three successive editions of the same work, although the first edition was published under a different title; the "third" edition was printed twice—in 1836 during Bell's life, and in 1844 after his death.

These three editions are:

In 1824. First.—*An exposition of the natural system of the Nerves of the Human Body. With a republication of the papers delivered to the Royal Society on the subject of the Nerves*, by Charles Bell, Professor of Anatomy and Surgery to the Royal College of Surgeons; Teacher of Anatomy in the School of Great Windmill Street; and Surgeon to the Middlesex Hospital. 8vo, London. Printed by Eyre & Spottiswoode, 1824.

In this volume the Introduction of sixty-six pages belongs to the year 1824. The paper "On the Nerves," etc., from the *Philosophical Transactions* of 1821, occupying pp. 67 to 180, contains many alterations from the original version, none of which are acknowledged or even hinted at.

This is the most important of the three editions; it shows as clearly as possible the transformation of Bell's views on the fifth nerve caused by the publications made by Magendie in the *Journal de Physiologie* of 1822 and 1823.

In 1830. Second.—*The Nervous System of the Human Body; embracing the papers delivered to the Royal Society on the subject of the Nerves*, by Charles Bell, F.R.S., 4to, London, 1830. Published by Longman, Rees, Orme, Brown & Green, Paternoster Row; and J. Taylor, Upper Gower Street.

Dedicated to the King, with a preface of thirteen pages and a note for which Bell "is indebted to a pupil" (*i.e.* Alexander Shaw) asserting the claim of Bell against Magendie. The *Phil. Trans.* papers of 1821, 1822, 1823 (2), 1826 and 1829 are reprinted with further modifications. The paper of 1829 —which consists substantially of a repetition of those of

1821 and 1822 *plus* the information gathered by Bell from the publications of Magendie and of Mayo—is interpolated between them. The order of the papers as given in this republication is 1821, 1829, 1822, 1823 (2), 1826.

The *first part* of the introduction (pp. 1 to 13) is new. The *second part* of the introduction (pp. 14 to 21) is substantially as in 1824; a footnote at p. 14 says that it was "written as introductory to the first edition." This footnote thus fixes what is otherwise left vague, that by first edition Bell means 1824 and by second edition he means 1830. ("Third edition" is prefixed to the publication of 1836 and to that of 1844.)

In 1836. THIRD.—*The Nervous System of the Human Body; as explained in a series of papers read before the Royal Society of London.* With an Appendix of Cases and Consultations on Nervous Diseases, by Sir Charles Bell, K.G.H., F.R.S.S.L. & E., Professor of Surgery in the University of Edinburgh, retired member of the Council and Professor of Anatomy and Surgery, Royal College of Surgeons of London, Fellow of the Royal College of Surgeons of Edinburgh, and late senior Surgeon of the Middlesex Hospital. Third edition. Edinburgh: Adam & Charles Black; Longman, Rees, Orme, Brown, Green & Longman, London, MDCCCXXXVI.

Reprinted in 1844 with three additional papers on the nerves of the encephalon. London: Henry Renshaw, 356 Strand, MDCCCXLIV.

A note precedes the preface of this edition to the following effect:

"These papers, read in succession, and at intervals according as the author, engaged in an arduous profession, could find time for the prosecution of such subjects, present a series which requires little comment. The evidence of originality is open to every unprejudiced mind. The author has neither been turned aside nor hurried into premature conclusions. The principle which directed him in these inquiries is stated in the very beginning, and step by step the reader may follow the observations and dissections by which the System has been made out."

The Appendix—pp. 243–451—containing *Consultations and Cases illustrative of the facts announced in the preceding papers* is the same in the two third editions of 1836 and 1844. The last ten pages are occupied by an expansion of the historical note

for which Bell acknowledges himself as indebted to a pupil (A. Shaw).

The edition of 1836 contains two *Phil. Trans.* papers of 1834 and 1835 and an *Additional Appendix* which includes a *Phil. Trans.* paper of 1832 ("On the Organs of the Human Voice"). Three short papers on the nerves of the encephalon as distinguished from those arising from the spinal marrow (from the *Transactions of the Royal Society of Edinburgh*, 1838) and a Plate XVI. are added to the 1844 third edition, which otherwise is identical with the third edition of 1836. None of these additions of 1836 and 1844 contains any matter bearing upon the present subject. The historical note by Shaw is of no value.

Bell's First Paper, 1821

Of Bell's six papers, the first, in 1821, dealing with the nerves of the face, is the most important, intrinsically as to its contents, extrinsically as to its relation to his other writings. The sixth, being his second paper on the nerves of the face, is of entirely extrinsic value; intrinsically it contains nothing new belonging to Bell; it merely incorporates new information derived from the publications of Magendie and of Mayo.

The reception of Bell's first paper, as may be gathered from his letters, was the source of considerable satisfaction to its author. He says of this paper in a letter to his brother of November 1821 that "It gives me a power of doing what I choose now, and will hereafter put me beside Harvey," and a little later,[1] in June 1822: "My discoveries have made more impression in France than here."

This was perfectly true. Magendie, as was his custom towards every physiologist whose work had appeared to him to be of value, published in the first and second volumes of his *Journal de Physiologie* (vol. i. p. 384, vol. ii. pp. 66, 363) an account by John Shaw of Bell's work on the fifth nerve and a translation of the experimental portion of Bell's actual paper. This was assuredly a handsome proceeding on the part of Magendie, which Bell accepted as a special tribute to his own merits but which Magendie paid at the same time to many other authors—to Broughton (vol. i. p. 120), to Rolando (vol. iii.

[1] Letters, p. 272.

p. 95), to Brodie (vol. iii. p. 93), to Mayo (vol. iii. p. 345) and to Christison (vol. iii. p. 274); as already stated above, he was unduly generous in his publication of Bell's puerile claim to priority about the roots preferred through the elder Shaw (vol. ii. p. 370).

Before considering this first paper in detail, it is necessary to bring clearly to mind the part played by the physiology of the fifth nerve in the development of Bell's claim.

It is a matter of common knowledge to-day that the fifth nerve consists of two principal parts—a larger ganglionated portion which is sensory, a smaller non-ganglionated portion which is motor. These two parts, as was well known to anatomists before Bell was born, are analogous with the posterior ganglionated and anterior non-ganglionated roots of the spinal nerves.

Bell and his adherents claim that this double structure and function of the fifth nerve and its analogy with a spinal nerve were discovered by Bell; also that the cardinal distinction between motor and sensory nerves, imperfectly established by his experiments of 1811 on the spinal roots, was finally and firmly established in 1821 by his experiments on the fifth nerve.

The stress laid by Bell and by Shaw from 1824 to 1839 upon the physiology of the fifth nerve is thus quite intelligible. Bell in 1811 had obviously neither looked for nor found any distinction between motor and sensory functions on the spinal roots and did not, therefore, in 1811 anticipate Magendie's discovery of 1822. Did he, however, in 1821 find the distinction between motor and sensory functions of the analogous roots of the fifth nerve? If he did this, he is clearly entitled to share with Magendie the honour of the cardinal discovery of the distinction between motor and sensory nerves. Therefore we examine with special interest his first paper on the facial nerves in the *Philosophical Transactions* of 1821. And as a matter of course we examine the original paper, which indeed is accessible enough. When we do so, we are surprised to find that the most careful examination fails to bring out any evidence of a distinction made by Bell between sensory and motor functions of the two parts of the fifth nerve. On the contrary, it becomes evident to us that in 1821, as far as can be understood from his somewhat sketchy language, Bell regarded the main portion of the fifth nerve (altogether apart from its minor portions) as both motor and sensory.

This is all the more surprising inasmuch as every one of Bell's publications after 1822 and nearly every English text-book of Physiology from 1830 to 1910 repeats the statement that Bell said that the major portion is sensory and the minor portion motor.

We begin, therefore, to look a little more closely into the "republications" of his original papers issued by Bell in 1824, 1830 and 1836. And now the matter is clear enough. The "republished" differ from the original papers and practically the whole literature of the last eighty years is based upon one or other of the "republished" versions. The differences introduced by republication are gradually progressive in the three successive editions of Bell's *Nervous System.* The most important and the least conspicuous are those made in 1824. In the subsequent editions of 1830 and 1836 they are maintained and supplemented by further additions to the text, which are acknowledged and therefore legitimate, although generally inaccurate. But the morally important and indefensible alterations are those unobtrusively made in 1824, immediately after Magendie's discovery, whereby the paper of 1821, as republished in 1824, is made to convey a totally different meaning. The most important portions to be collated are pp. 408 to 414 of Bell's original paper on the nerves of the face in the *Philosophical Transactions of the Royal Society* for 1821 and pp. 93 to 109 of the republished paper in *An Exposition of the Nervous System* published by Charles Bell in 1824. The title-page of this work has upon it the legend, "With a republication of the Papers delivered to the Royal Society on the subject of the Nerves." At p. 66 of the introduction Bell says: "I shall now lay before the reader the papers which I presented to the Royal Society on this subject and in the order in which they are printed in the *Philosophical Transactions.*" Nothing is said by Bell in this publication of 1824 of any change of views nor of any alteration of text.

Space will hardly allow of the reproduction of these two passages in this article but a single short extract from the original in 1821 and the republication in 1824 will suffice to show the slightness and the gravity of the differences between the two versions. It must be borne in mind, in comparing the two passages, that Magendie's discovery was published in 1822.

The original passage of 1821 (*Phil. Trans. R.S.* p. 409) reads thus:

"*Of the Trigeminus or Fifth Pair*

"From the nerve that comes off from the anterior ganglion of the leech, and which supplies its mouth, we may trace up through the gradations of animals a nerve of taste and manducation, until we arrive at the complete distribution of the trigeminus in man (see Plate XXX. B.C.D. which are its three grand divisions to the face). Here in the highest link, as in the lowest, the nerve is subservient to the same functions. It is the nerve of taste, and of the salivary glands, of the muscles of the *face and* jaws, and of common sensibility. This nerve comes off from the base of the brain in so peculiar a situation, that it alone of all the nerves of the head receives roots both from the *medullary process of the cerebrum and of the cerebellum.* A ganglion is formed upon it near its origin, though some of its filaments pass on without entering into the ganglion. Before passing out of the skull the nerve splits into three great divisions, which are sent to the face, jaws, and tongue. Its branches go minutely into the skin and enter into all the muscles, and they are especially profuse to the *muscles which move the* lips *upon the teeth.*"

As republished in 1824 (*An Exposition*, etc., p. 96) the passage reads:

"*Of the Trigeminus or Fifth Pair*

"From the nerve that comes off from the anterior ganglion of the leech, and which supplies its mouth, we may trace up through the gradations of animals a nerve of taste and manducation, until we arrive at the complete distribution of the fifth, or trigeminus in man (see Plate III., in which there are its three grand divisions to the face). Here in the highest link, as in the lowest, the nerve is subservient to the same functions. It is the nerve of taste, and of the salivary glands; of the muscles of the jaws, and of common sensibility. This nerve comes off from the base of the brain in so peculiar a situation, that it alone, of all the nerves of the head, receives roots both from the medullary process of the *cerebrum* and of the *cerebellum.* A ganglion is formed upon it near its origin, though some of its filaments pass on without entering into the ganglion. Before passing out of the skull, the nerve splits into three great divisions, which are sent to the face, jaws, and tongue. Its branches go minutely into the skin and enter into all the muscles, and they are especially profuse to the lips."

There are two significant differences between these two versions.

1. Originally—*i.e.* in 1821—"The nerve supplies the muscles of the face and jaws." Subsequently—*i.e.* in 1824 and henceforth—"The nerve supplies the muscles of the jaws."

2. Originally—*i.e.* in 1821—"Its branches are profuse to the muscles which move the lips upon the teeth." Subsequently—*i.e.* in 1824 and henceforth—"Its branches are profuse to the lips."

In short, for Bell in 1821 the fifth nerve, which resembled a spinal nerve by its possessing a cerebral and a cerebellar root, subserved the ordinary motions and sensations of the whole face by virtue of its cerebral root. Whereas in 1824, *i.e.* after the work of Magendie on the spinal roots and of Mayo on the nerves of the face, the fifth nerve was sensory to the face by its ganglionated portion and motor to the muscles of mastication by its non-ganglionated portion.

If any one should be tempted to excuse Bell's neglect of accuracy as the pardonable inadvertence of a man of genius, intent upon the discovery and neglectful of petty conventions, let him consult the *Narrative of the Discoveries of Sir Charles Bell in the Nervous System*, in which Alexander Shaw, as Bell's mouthpiece, fills upwards of two hundred tedious pages with a minute account of what Mayo and Magendie wrote and did not write about what Bell and John Shaw wrote and did not write in this or that month of 1821, 1822 and 1823. Bell was intolerant enough of other people's "inaccuracies" or for what might be represented as such.

We have seen what Bell says about Magendie and we are naturally curious to learn what Magendie says about Bell.

Magendie's reply, in words and in conduct, is most dignified. He prints the reclamation made for Bell in the following issue of his *Journal* (vol. ii. p. 370), quotes Bell in full and believing the "experiment" alluded to by Bell to be a physiological experiment on a living animal—which it was not—gives to Bell a credit that he did not deserve. And while Bell in 1823 uses the most injurious language towards him, he continues to find place in his *Journal de Physiologie* for Bell's work when he considers it to be of value. In the tenth volume of the *Journal*, in 1830, he prints in full the translation and illustrations of Bell's second paper on the Nerves of the Face of 1829, with the

following prefatory footnote, than which it would be difficult to imagine anything more just or more generous or more scathing:

"Je me suis toujours fait un devoir de présenter aux lecteurs de ce Journal les travaux anatomiques de M. Ch. Bell. Son esprit investigateur bien qu'un peu spéculatif, sa grande habileté dans l'art de disséquer, et son rare talent pour le dessin, lui assureront toujours une place distinguée parmi les anatomistes de notre époque. Pourquoi faut-il que ce savant nuise à ses travaux, se nuise à lui-même en ne rendant pas à ses émules la justice qui leur est due? Pourquoi conserve-t-il ce patriotisme barbare qui repousse tout ce qui n'est pas du pays? Pourquoi garde-t-il des prétentions à des découvertes qu'il n'a pas faites? Sans doute parce que tel est son caractère, et qu'il n'est pas facile de se changer, quand même on en sentirait le désir. D'ailleurs ce travers, que notre franchise se permet de blâmer en lui, est peut-être le mobile qui l'excite au travail, et alors voudrions-nous réellement qu'il ne l'eût pas?"

The Portio Dura of the Nerve in 1821 and 1824

Bell's emendations of text between 1821 and 1824 are not confined to the fifth nerve—corresponding emendations are introduced at the same time with reference to the *portio dura* of the seventh. This nerve in 1821 is motor and sensory, in 1824 it is only motor, in association with the respiratory movements of the face. In the case of the seventh as in that of the fifth the alterations of text are very slight but none the less significant. They also occur in descriptions of experiments and of figures.

In 1821, p. 411, "branches of the seventh are sent *to the membrane of the nose and* to the muscles at the back of the palate."

In 1824, p. 99, the words I have italicised are omitted and "branches of the seventh are sent to the muscles at the back of the palate."

In 1821, p. 411, "Its branches penetrate to the skin accompanying the minute vessels of the cheek" and at p. 423 in the explanation of Plate XXX. there is the following description:

"*c.* A large division of the respiratory nerve which goes to the muscles of the mouth and to the integuments of the cheek."

In 1824 the description disappears. In 1830 the passage disappears from the text.

And the experiment on the asses follows suit; they are altered to fit the altered text. The thrown ass of 1821 (p. 413) whose *portio dura* had been divided on one side "ate without the slightest impediment."

In 1824 Bell tells us about this same ass that the motion of eating would have been obviously defective had the same nerve of the opposite side been cut.

In 1830 (p. 73) no more is said about the ass eating at all.

This was the first or *portio dura* ass, not to be confused with Bell's second or superior maxillary ass, which also underwent transformation.

In 1821 (p. 413) this second ass, after division of the supra-maxillary nerve, had "such obvious loss of motion of the lips in eating that it was thought a useless cruelty to cut the other branches of the fifth." By 1824 (p. 107) Bell had realised that what he had attributed in 1821 to loss of motion was in fact due to loss of sensation. And in 1836 (p. 52) Bell remembers that "In my first experiments the loss of sensibility of the lips was so obvious that it was thought a useless cruelty to cut the other branches of the fifth."

Now as Bell never claims to have made any but these *first* experiments, his allusion to subsequent experiments by which his views might have been corrected is mere verbiage. And seriously speaking no man of science could ever have tampered with his own reports of his own experiments as Bell did: his testimony is worthless.

Bell's Second Paper, 1822.—*The Respiratory Nerve of the Face*

Bell's second paper to the Royal Society deals entirely with his theory of a distinct respiratory system of nerves and more especially with the *portio dura* of the seventh or respiratory nerves of the face. It is republished in the *Exposition* of 1824 with the slight alterations that might be expected from the alterations made to the fifth nerve. In 1821 the fifth is motor-and-sensory and the seventh (*portio dura*)—in a peculiar fashion as a superadded or respiratory nerve—is also motor-and-sensory. In 1824 the fifth is sensory and the seventh motor.

The principal idea brought forward by Bell in this second communication to the Royal Society of 1822 is that of the respiratory system of nerves arising from a lateral column

intermediate between the anterior and posterior columns. This idea, although repeatedly insisted upon by Bell as fundamental, was not favourably received by physiologists, with one notable exception, viz. Gaskell, who took it as the foundation of his Splanchnic System of Nerves connected with the lateral tract and with Clark's column and distributed to "visceral" muscles, among which curiously enough Gaskell places the muscles of facial expression. It would take us too far afield to discuss these considerations which are hardly germane to the simpler issue now before us; nevertheless as indicating a favourable opinion of Bell's position in this respect at variance with the unfavourable opinion I am now presenting, I think it may be right to indicate by a short quotation from Gaskell's writings that a distinguished physiologist does actually in 1886 attach high value to Bell's work. Obviously, however, I do not subscribe to the opinion I am about to quote:

"As regards this latter question (as to the double character of each of the two roots, anterior and posterior) the answer has already been given by one of the greatest of English physiologists: Charles Bell, in his description of the nerves of the body, was the first to draw attention to the triple nature of the nerve roots throughout the central nervous system. He divided them into three sets: (1) anterior, containing the fibres of common voluntary motion; (2) posterior, containing the nerves of sensation; (3) lateral or respiratory, containing nerves which excite motions dependent on or related to the act of respiration. He says: 'I imagine that the same column or tract which gives origin to the fourth, seventh, glosso-pharyngeal, pervagum, and spinal accessory nerves is continued downwards along the lateral parts of the spinal marrow, and that it affords roots to the spinal nerves, constituting them respiratory nerves as well as nerves of motion and sensation; and that it especially supplies the roots of the diaphragmatic nerve, and the external respiratory nerve.' Charles Bell himself was, I understand, more proud of this conception of special lateral and respiratory roots than of his famous functional separation of the anterior and posterior roots. Physiologists have doubtless failed to follow up this clue thus given by Bell because the full bearing of this triple arrangement of the nerve roots was not immediately evident like the separation of anterior motor from posterior sensory roots. Now, however, we can see the importance of his conception; we see that this lateral tract of nerve roots, these so-called respiratory nerves are the same as the non-ganglionated part of the splanchnic root. The significance of this agreement between Bell's conception and

mine is apparent on the face of it; by respiratory nerves, he meant, as he himself describes, those nerves which are dependent on or related to the act of respiration. The muscles in question included those of deglutition, of phonation, of smelling, etc., as well as the more purely respiratory muscles."

Bell's third and fourth papers to the Royal Society (1823) deal with the muscles and nerves of the eyeball and orbit but from our present historical standpoint they are of significance only on account of the allusions they contain to the physiology of the fifth nerve and to Magendie's work of 1822. The first part of this double paper (March 20) deals principally with the motions of the eye and hardly concerns us; it contains no allusion to motor and sensory functions of nerve nor to the fifth nerve nor to Magendie.

The second part (June 19) begins with an account of the function of the ophthalmic division of the fifth nerve, which it describes as being that by which—

"the common endowment of sensibility is bestowed upon the membranes and surfaces of the eye." Bell now says that the "trigeminus or fifth nerve bestows upon all the surfaces of the head and face, external and internal, that sensibility which is enjoyed by the rest of the body through the spinal nerves." He says that "it has been shown in a preceding paper, by experiment, that on dividing the branch of the fifth nerve to the cheek and lips the skin was deprived of sensibility although in possession of other nerves and enjoying muscle activity."

We have to remember that Bell is now writing in 1823, *i.e.* after Magendie of 1822; his statement that he knew all this about the fifth nerve in 1821 is not borne out by reference to the paper of 1821, when he imagined that the fifth nerve was motor-and-sensory. And now we understand the alterations alluded to above in the passage about the fifth nerve as republished in 1824.

The next section in this paper on the eye deals further with nerves performing the involuntary motions, which he associated with the respiratory nerve of the face; it ends up with a footnote containing an indirect claim to having demonstrated distinct functions in the separate roots of the spinal nerves (p. 294). The next section deals with the fourth nerve, which

Bell reckons among the respiratory nerves; he alludes to muscular relaxation producible through the influence of the fourth nerve and in a footnote (p. 296) describes an experiment to demonstrate elongation of an extensor muscle in association with the contraction of the opponent flexor muscle. The reader is left in doubt as to the manner in which this experiment can have been performed. In his summary at the end of this paper, on p. 300, he gives for the first time a clear account of the fifth nerve as the spinal nerve of the head with a double origin like a spinal nerve and a double function, bestowing sensibility to the head and face, to the skin, to the surfaces of the eye, to the cavities of the nose, the mouth and tongue and motion to the muscles of the jaws and mastication. He describes the *portio dura* of the seventh as the motor nerve of the face and eyelids and the respiratory nerve and that on which the expression of the face depends; all this, it is to be remembered, occurs in the second part of the paper on the nerves of the orbit of June 1823, not in the paper on the nerves of the face of 1821; and as if to make quite sure of proving to his reader that it is in consequence of Magendie's work of 1822 that Bell 1821 has been transformed into Bell 1823 as regards the fifth and seventh nerves, he concludes this second paper on the nerves of the eye by saying that in a foreign review of his former papers his results have been considered as a further proof in favour of experiments and protests that experiments have never been the means of discovery and invokes the examples of our own great countrymen, the Munros and the Hunters as distinguished from the physiologists of France who borrow from us and follow up our opinions by experiments; in a footnote he clenches the case against himself by saying "see the experiments of Monsieur Magendie on the distinction between the roots of the spinal nerves" (p. 307).

Bell's fifth paper, on the "Nervous Circuit," was published in 1826, when the distinction between motor and sensory fibres, established by Magendie, had become common property and it had been made clear that the fifth nerve was mainly sensory, the seventh nerve mainly motor. He had previously described the fifth as distributed to muscles as well as to the skin in the belief that it was a sensory-and-motor nerve. And he evidently felt

the difficulty of accounting for this muscular element in the distribution of the nerve. He now says (p. 164) that

"for a time I believed that the fifth nerve, which is the sensitive nerve of the head and face, did not terminate in the muscles, but only passed through them to the skin."

But he implies that he now admits that it does terminate, as he first thought, in the muscles. And since now it is certainly sensory, its ramifications in the muscle must be the sensory nerves of muscles.

The principal object of this paper of 1826 is therefore

"to demonstrate *that where nerves of different functions take their origin apart and run a different course, two nerves must unite in the muscles, in order to perfect the relations betwixt the brain and these muscles*" (p. 164).

He opens his "demonstration" or argument by saying that

"it may be within the recollection of the Society that my first paper [of 1821] showed the difference of the nerves of the face; by dividing one nerve sensation was destroyed, whilst motion remained; and by dividing the other motion was stopped, whilst sensibility remained entire."

(This is in fact an appropriation to himself of the lion's share of a discovery in which he no doubt participated but to which the principal contributors were Magendie and Mayo.)

He then proceeds to argue that "the profuse supply to muscles of nerves whose office it is to convey sensation, in addition to motor nerves through which motion is excited," signifies that "there must be a sense of the condition of the muscle communicated to the brain as well as energy proceeding *from* the brain *towards* the muscles;" and on p. 170 as the conclusion of his "demonstration" states that

"*between the brain and the muscles there is a circle of nerves; one nerve conveys the influence from the brain to the muscle, another gives the sense of the condition of the muscle to the brain.*"

This conclusion of Bell's demonstration is the conclusion of an argument in which the essential data were supplied by the definite experiments of Magendie and of Mayo and of which the general idea had been most clearly outlined by Walker in

1815 when he imagined a "circulation in the nervous system from and to almost every point of the body, ascending to the cerebrum by a nerve of sensation and returning by a nerve of volition."

That Walker in 1815, *i.e.* before Magendie's discovery of 1822, pictured the sensory direction as up the anterior and the motor direction as down the posterior parts of the circuit does not in any way obscure the idea of the nervous circuit. What Bell added to this idea in 1826 was the specific picture of a muscular circuit, by sensory nerves from muscle to brain and by motor nerves from brain to muscle.

Bell's sixth paper (1829)—his second paper on the nerves of the face—is a revised and greatly improved edition of his first paper of 1821. The improvements are entirely consequent upon the distinction made by Magendie between motor and sensory nerves and upon the experiments published by Mayo on the nerves of the face; they are also in part due to Bell's better understanding of the anatomy of the fifth nerve as described in Bellingeri's thesis of 1818. Both in the text and in accompanying figures the essential points as regards the physiology of the spinal roots and of the two parts of the fifth nerve are clearly and correctly given. Assuredly if this second paper of 1829 had been written in 1821, Bell's title to the discovery of the distinction between the motor and sensory functions of the spinal roots and of nerves in general would have been clear. But as a matter of fact the paper contains no additional results of Bell's work, the improvements are in substance due to the incorporation of the results obtained by Magendie and by Mayo in 1822 and 1823.

By the sixth paper Bell has returned to his point of departure and completed the circle of his most active period of physiological publication. After 1829 there is nothing further from his pen worth mentioning; three further papers appeared indeed in the *Phil. Trans. of the Royal Society*, in 1832 "On the Organs of the Human Voice," in 1834 and in 1835 "On the Functions of some parts of the Brain" and "On the Relations between the Nerves of Motion and of Sensation." After 1836 when Bell returned to Edinburgh as Professor of Surgery, several short papers from him "On the Nerves" were communicated to the *Transactions of the Royal Society of Edinburgh.* None of these

papers contains anything fresh bearing upon the functions of the roots or of the nerves of the face. They all contain repetitions of his claims as the Discoverer of the Nervous System—natural enough on the part of a man of his age and standing but without any other reason or excuse. Thus in 1840, an otherwise insignificant paper by Sir Charles Bell in the *Philosophical Transactions* entitled "On the Nervous System," serves merely as the vehicle for the following careful and misleading passage in which Bell reiterates once more his now fully expanded claim:

"*The Spinal Nerves*

"In the earliest part of my investigations I performed experiments on the roots of these nerves. I exposed the spinal marrow, separated the distinct roots and found that the anterior root, on being irritated, excited motion, while the posterior root did not excite the muscles. By inference, and by comparing the spinal nerves with those of the encephalon, I was at length led to conclude that the anterior root is provided for motion, and the posterior for sensation; and that the spinal nerves instead of being common nerves, are in truth combined of roots, one of which gives motion and the other sensation."—*Phil. Trans. R.S.* 1840, p. 245."

To recapitulate—Bell's title as a physiologist rests entirely upon the six papers published in the *Philosophical Transactions of the Royal Society* from 1821 to 1829, which may be succinctly indicated as follows:

1. On the Nerves of the Face . . July 12, 1821, p. 398.
2. On the Nerves of Respiration . . May 2, 1822, p. 284.
3. On the Motions of the Eye . . March 20, 1823, p. 166.
4. On the Nerves of the Orbit . . June 19, 1823, p. 289.
5. On the Nervous Circle . . . February 16, 1826, p. 163.
6. On the Nerves of the Face (2nd paper) May 28, 1829, p. 317.

Before 1821 and after 1829 there is no valid document in support of Bell's claim to the discovery of the separation between motor and sensory nerves. His booklet of 1811, so far from containing evidence in support of his claim, conclusively proves that his ideas at that time were entirely beside the mark. His papers of the period 1832–44 are not worth quoting.

Of the six papers which we have carefully examined in the preceding pages it is evident that the first and the sixth are the

most important. The sixth paper is excellent and would suffice to establish his claim if it were not in the main composed of borrowed matter. The first paper in its original unamended form in 1821 is unsatisfactory and in its republished form after 1822 is in substance a falsified document.

There is no escape from the conclusion that Bell's claim as a discoverer was a carefully fabricated claim and that the discovery of the distinction between motor and sensory nerves belongs entirely to Magendie.

LIFE WITHOUT OXYGEN

THE ANAEROBIC BEGINNINGS OF LIFE

By CARL SNYDER

In a previous essay in this journal[1] on the Physical Conditions at the Beginnings of Life, it was briefly suggested that these beginnings must have been in the absence of uncombined oxygen, that is, in Pasteur's phrase, anaerobic.[2] At least five or six convergent lines of evidence lead to this unexpected conclusion. These are:

1. That what appear to be the simplest forms of life are anaerobic still and that the majority of lower organisms, both plant and animal, can live under anaerobic conditions more or less continuously.

2. That the fundamental chemical processes of the cell in all organisms, even the highest, are anaerobic, phenomena of oxidation being of secondary or ulterior importance.

3. That the relations of anaerobic and aerobic life are genetic and that we have clear evidence of the gradual evolution of the latter from the former, an increasing need of oxygen accompanying an increasing complexity of chemical and morphological organisation.

4. That this increase in need of oxygen as complexity increases is paralleled in the growth of the individual organism, the ability to endure complete abstraction of oxygen varying inversely with age and size.

5. That the oxygen of the atmosphere appears to have been formed exclusively by plant action. In the beginning there was probably no oxygen free: if there had been any, it would have been very quickly absorbed by the unoxidised substances of the earth's crust or the quantity would have been so small as to be practically negligible.

[1] Science Progress, April 1909, p. 579.

[2] Cf. Weinland, *Zeit. f. Biol.* vols. **42** and **43**; E. J. Lesser, "Das Leben ohne Sauerstoff," *Ergeb. d. Physiol.* 742, **8**, 1909.

I. *The Facts of Anaerobiosis.*—So incessant and so absolute is the need of oxygen by the human organism and by animal organisms in general, that in the half-century following the momentous work of Lavoisier the belief in oxidation or "combustion" as a primary process of life came to be an *idée fixe* among physiologists, in spite of the fact that Spallanzani,[1] who was almost contemporary with Lavoisier, had shown that infusorial animalcules can thrive when the supply of oxygen is reduced to a minimum (Spallanzani operated with air pressures of from one to three inches) and that the production of carbonic acid gas continues, in some cases without diminution, when the supply of oxygen is entirely cut off.

But everyday familiar experience proved too strong and so it was that the proof came as a shock when the existence of beings was established which not only did not need oxygen but to which the slightest trace of oxygen is as fatal as lack of it is to us. Had the discovery been announced with anything less than the preponderant authority of Pasteur it must have long remained unrecognised. Pasteur's first communication was published in 1861 and was entitled "Animalcules infusoires vivant sans gaz oxygène libre et determinant des fermentations."[2] In a footnote he remarks: "I propose, with all reserve, these new words, *aerobies* and *anaerobies*, to indicate the existence of two classes of primitive beings, one incapable of living save in the presence of oxygen; the other able to multiply indefinitely without contact with this gas."

During the half-century that has followed this announcement, this capacity of existing, for a time at least, in the complete absence of uncombined oxygen—facultative anaerobiosis—has been shown to be of wide occurrence in both the plant and animal kingdoms.[3] On the other hand, the possibility of complete and unconditional anaerobic existence has been repeatedly denied; needless to say, it is one of the most difficult things in the world to establish the absence of oxygen. Thus Beijerinck[4] was able

[1] *Memoirs of Respiration*, English translation, 1804.

[2] *Comptes rendus*, 344, **52**, 1861.

[3] For an admirable summary, with full literature, cf. E. J. Lesser, "Das Leben ohne Sauerstoff," *Ergeb. d. Physiol.* p. 742, **8**, 1909; excellent historical reviews may be found in Jacques Loeb, *Pflueger's Arch.* 249, **62**, 1895; H. Winterstein, "Atmung d. Geweben," *Zeit. f. Allg. Physiol.* 315, **6**, 1906; J. Kürsteiner, *Cent. f. Bakt.* II. 109, **19**, 1907.

[4] *Arch. Neerland*, Series II. 397, **2**, 1899; 131, **9**, 1904.

to show that, in the case of certain forms supposedly anaerobic, for example the butyric acid ferments, the minutest trace of oxygen sufficed during a long series of generations. Beijerinck, therefore, proposed to divide living organisms into two classes —those which thrive best when the oxygen tension is at a maximum and those which thrive best when it is at a very low tension. The former he called aerophile, the latter micro-aerophile. This conception seemed to fit the facts so well that it was generally accepted for a time by the most careful observers, among them Fermi, A. Fischer, Chudiakow and others.[1] But the discovery and investigation of the light-producing or photo-bacteria revealed a test for the presence of oxygen of such sensitiveness as to disclose the most minute trace. With the aid of this last and the development of a technique of quite extraordinary refinement, it has been possible, notably through the researches of Omelianski, Burri and latterly by the decisive work of Kürsteiner,[2] to establish the correctness of Pasteur's early observations.

To those to whom the law of continuity or curve of probabilities is a constant mental companion, this final demonstration seemed a foregone conclusion. The fact that in passing from organisms like the higher animals to which a constant supply of oxygen is a necessity, every gradation in the need of oxygen was to be met with down to a point at which the utmost refinement of technique was required to discover that it was of use at all, left little reasonable doubt in the philosophic mind that beyond this point the absolute anaerobe must exist.[3]

Why now should we regard these anaerobic bacteria as the lowest, the most primitive and therefore in all probability the earliest of all living organisms known? For very much the same reason that we speak of the plant as "lower" in the scale of life than the animal. In general, at least, the higher types of animals are able to exist only by means of preformed organic products. The plant is able to derive its food directly from the soil and the air. It is evident therefore that a great land flora must have preceded any notable development of a land fauna.

Further, it is known that prior to the development of an

[1] For full literature, cf. Omelianski, in Lafar, *Handb. d. tech. Mycol.* 2nd edition, vol. ii. 576, 1907.

[2] *L.c.* with full literature.

[3] For a full discussion, cf. Euler, *Pflanzenchemie*, vol. ii. p. 152 ff.

extensive land flora there must come a great bacterial population, both to form and to prepare the soil. This is evident from what happened after the Krakatoa explosion in 1884.[1] So far then as life on land is concerned, an extremely primitive microscopic life is apparently antecedent to all other forms.

The same is true of the sea. In recent work, A. Pütter[2] has endeavoured to show that the floral and primitively synthetic organisms of the sea are quite inadequate to support the astounding abundance of life which, little by little, we are coming to learn exists there—a volume so great that compared with it the total amount of land and aerial life is an almost negligible quantity. Pütter, therefore, endeavours to show that the sea is a vast, though very weak, organic solution. His results have not been confirmed and have even been directly called in question,[3] though on the face of the evidence some such conception as his seems to be necessary to many.[4] But whether or no the destructive metabolism of the sea be less complete than that on the land, it is still obvious that a vast floral life, directly utilising solar energy, is a condition of a development of oceanic animal life such as we know exists; such a flora is found in the Plankton. It is evident that the algæ, the desmids and their like must have preceded, in point of time, the more elaborate animal types. Side by side with these is to be found in sea water the same teeming bacterial life as in the soil; moreover, for the most part, marine bacteria are of the same types and varieties as soil bacteria.[5]

It is amid these prototypes of life, both in the sea[6] and in the soil, that the anaerobic bacteria are found. Like all bacteria and the most primitive types in general, they are neither plant nor animal in any clear sense of the word. Some, like the plants, derive their nutrition directly from inorganic materials; others live on the dead organic substance of plants and animals; whilst others are able to exist only as parasites inhabiting the tissues or cavities of living organisms. Finally, some live now in the one way, now in the other. They contain no chlorophyll; they

[1] Treub, cited from Euler, *l.c.* p. 140.

[2] "Die Ernahrung der Wassertiere," *Zeit. f. allg. Physiol.* Bd. vii. Heft 2 and 3, 1907.

[3] Cf. Lesser, *l.c.*; M. Henze, *Pflueger's Arch.* 478, **123**, 1908.

[4] Cf. Johnstone, *Life in the Sea*, p. 221 ff. (London, 1908).

[5] Johnstone, *l.c.* 253 ff.

[6] Keutner, *Wissensch. Meeresunters. Kiel. Com.* Bd. viii. 1905.

need no sunlight. Some are fixed but others are free swimming organisms. But whereas only a few of the bacteria and practically none of the plant forms—the case of the Schizophyceæ is still rather doubtful[1]—are able to assimilate gaseous nitrogen, it is among the anaerobic bacteria that the most typical nitrogen fixers are found.

Consider the implication. The anaerobic organism is evidence of the fact that gaseous oxygen is not essential to the synthetic changes which accompany life. It is only as we rise in the scale of life that the need of oxygen becomes obvious. On the other hand, the capacity of assimilating nitrogen exists only in the lowest forms and as we ascend the scale is very quickly lost. Considering how essential the element nitrogen is in the chemistry of life and the established non-essential character of gaseous oxygen, we seem driven to the conclusion that the most primitive and therefore the earliest form must have been a nitrogen fixer to which a supply of uncombined oxygen was immaterial.

It is true that *Bacterium clostridium* (Past.), the best studied of the organisms that assimilate nitrogen, requires for its nutrition a supply of glucose,[2] that is to say an organic product; whilst forms are known, like Nostoc, among the Schizophyceæ, which are able to fix at least minor quantities of nitrogen in an entirely inorganic culture medium, that is to say they do not require a ready-made supply of complex organic carbon compounds. It is for this reason that the Nostoceæ have been regarded by some as the more primitive form.[3] Euler observes:

"We have herewith every reason to seek among the Schizophyceæ the earliest inhabitants of the earth. Further support for this assumption is derived from repeated observations that these algæ are always the first colonists on sand, bare and humus soil and the like. A celebrated example is that afforded by the Nostoceæ, which Treub found as the earliest germinants on the sterile soil of Krakatoa, after the famous catastrophe."

But the ability to assimilate carbon from its simplest inorganic compounds, the carbonates, not only from carbon

[1] Heinze, *Cent. f. Bakt.* II. Bd. xvi.

[2] For an excellent review of Winogradski's work, cf. Jost, *Pflanzen physiologie*, 2te auf. 1908; also in English translation.

[3] Euler, *l.c.* Bd. ii. p. 140.

dioxide but even from the deadly monoxide,[1] is shared by many absolute anaerobes, even though it is not by *Bacterium pasteurianum*. Nostoc is apparently better able to propagate rapidly under the atmospheric conditions which now prevail but as we shall see it is at least extremely doubtful if these same conditions obtained in the earlier epochs of the earth's history.

The conclusion that the anaerobic nitrifiers constitute the more primitive form is supported by the behaviour of these and other low forms towards an oxygen supply. It is now a well-established fact that many, if not all, of the incontestably anaerobic bacteria may, under suitable conditions, be cultivated and reproduce rapidly under ordinary atmospheric conditions.[2] On the other hand, some at least of the supposedly absolute aerobes, as Rosenthal, Tarozzi and others have shown, if passed through a series of cultures in which the supply of oxygen is more and more attenuated, may at last be cultivated freely under completely anaerobic conditions.[3] In the ancient phrase they may be "adapted" to an anaerobic life.

It has long been known that there are intermediate forms which are indifferently oxygen users or abstainers, according as this element is present or absent; in other words, the oxygen habit is apparently one which may be readily assumed and readily put aside. On the other hand, there does not seem to be any clear evidence that a corresponding nitrogen habit may be acquired where it is non-existent. The ability to assimilate uncombined nitrogen seems to be an extremely primitive faculty, quickly put aside and finally lost as the complexity of the organism increases.

In the light of this conclusion and the fact that complete and enduring anaerobiosis is to be found only among the more primitive forms, it seems almost impossible to regard the faculty of living without oxygen as an "acquirement." We shall have decisive proof in the sequel that it is not. It is the ability to use uncombined oxygen which is the "acquirement"; and we seem therefore driven to the conclusion that the anaerobic was the more primitive and the earlier form.

[1] Koserer, *Cent. f. Bakt.* II. Bd. **16**, 1906. Further literature in O. Richter, *Die Badeutung der Reinkultur* (Berlin, 1907).

[2] G. Tarozzi, *Atti R. Accad. dei Fisiocratidi*, Sienna, serie 4, v. **17**, 1905; also vol. **15**, and 1906.

[3] G. Rosenthal, *C. r. Soc. Biol. Paris*, No. 25, 1906.

What is true of the bacteria is equally true of the protozoa.[1] Thus Bunge[2] has shown that many of the more primitive forms are either actively or conditionally anaerobic to a very considerable degree. Vinegar eels (*Anguillula aceti*) lived and displayed the liveliest sort of motions in complete absence of oxygen during seven days, leeches during three days, Ascaris five days, Hæmoptis two days, Clepsina six days, Nephillis two days, snails ten to fifteen hours, etc. Apparently the observations were carried out at room temperature; Pflueger,[3] however, has shown that the ability of the frog to resist the withdrawal of oxygen is inversely proportional to the temperature, so that it is probable that at the ordinary temperatures at which many of the slime worms live their need of oxygen is still less than Bunge's observations would lead us to suppose is the case.

Bunge's work has been continued by Weinland,[4] Pütter and Lesser[5] among others. Pütter[6] kept leeches alive during ten days; *Opalina* during twenty days; he also experimented with *Paramæcium caudatum*, *Colpidium colpoda*, *Opalina ranarum*, *Balantidium eotzoon*, *Nyctotheris caudiformis* and *Spirostomum ambiguum*. Pütter's conclusions were as follow:

"In their widespread independence of molecular (free) oxygen and in the dependence of the faculty of living anaerobically on the state of nutrition and the supply of nutriment, the protozoa present a close agreement with countless plants, whilst in the animal kingdom we have as yet few analogies. But this is doubtless due to the fact that we have as yet few experiments on the respiration of the lower animals and that these even have generally been interpreted so as to agree with the dogma of the invariable necessity of molecular oxygen" (*l.c.* p. 612).

Duclaux[7] speaks to the same effect when he remarks upon the part which pure *suggestion* has played in the formation of the ancient (but still largely dominant) theories of respiration.

[1] Cf. Prowazek, *Physiol. d. Einzelligen*, p. 50 (Leipzig, 1909).
[2] *Zeit. physiol. Chemie*, 48, **8**, 1883; 565, **12**, 1889; 318, **14**, 1890.
[3] "U. physiol. Verbrennung," *Zeit. f. ges. Physiol.* 1875.
[4] *Zeit. f. Biol.*; various papers in vols. **42, 43, 45, 48.**
[5] Lesser, *l.c.*
[6] "Die Atmung d. Protozoen," *Zeit. f. allg. Physiol.* 566, **5**, 1905.
[7] *Traité de Microbiologie*, t. i.

II. *The Fundamentally Anaerobic Character of the Vital Process.* —The first wide breach in the ancient dogma that oxygen played an essential part in vital metabolism came from the researches of Pasteur; his work was the first which distinctly showed that the vital process was to be correlated with the process of fermentation. It was he, indeed, who defined fermentation as "life without air," that is without oxygen as such. It is among the further ironies that though the long strife between the followers of Pasteur and the followers of Liebig, over the chemical nature of fermentation, was ended finally and decisively in 1897 by Buchner's epoch-making discovery of zymase, most biologists still cling implicitly to Pasteur's ideas of life in spite of the proof that Pasteur himself supplied that life and fermentation are one and the same thing.[1]

Pasteur showed that in the case of certain of the lower organisms the life process could be carried on in the absence of oxygen. But the generalisation of this fact, that anaerobiosis is fundamentally true of the vital processes in all organisms, was first clearly set forth, apparently, by Hoppe-Seyler. This conclusion was implied in the work of Spallanzani and indeed was distinctly stated by him in the latter part of the eighteenth century. This amazingly fertile and pioneering man had studied the production of carbonic acid in the case of living and dead snails "in azotic (nitrogen) and hydrogen gas and in common air." Summing up (memoir cited, p. 351) he said:

"I shall only conclude from these experiments that it is clearly proved that the carbonic acid gas produced by living and dead snails in common air did not result from the atmospheric oxygen, since an equal and even greater quantity was obtained in azotic and hydrogen gas."

Spallanzani was a full three-quarters of a century ahead of his time. It is instances like this that serve to remind us at times of the large amount of clear-sighted work which remains unrecognised.

Hoppe-Seyler's investigations were formulated in the first volume of his *Physiological Chemistry*, published in 1877. His cautious conclusion was stated in the following words:

[1] For historical review with full literature cf. Ahrens, *Das Gärungsproblem* (Stuttgart, 1902).

"Although the putrefactive processes have been set forth in complete parallel to the phenomena of life, it should not be understood thereby that the two are identical, only that any difference is as yet unknown" (*l.c.* p. 128).

Hoppe-Seyler based his view of the fermentative nature of respiration on the following facts:

1. In the animal organism, reduced substances such as urobilin, succinic acid, hippuric acid and so forth result from the introduction of quinolinic acid and the like and appear in the urine in company with unquestionable products of oxidation.
2. Many easily oxidisable substances may pass through the organism unoxidised.
3. Between the production of carbon dioxide and the consumption of oxygen there is no constant relation.
4. Although ozone is not and cannot be demonstrated in the organism, nevertheless a complete destruction of complicated organic compounds to carbon dioxide and water takes place, whilst we are able to bring about these same oxidations outside the body only very slowly and incompletely even with the aid of the strongest oxidising materials (*l.c.* p. 128).

Similar ideas were independently advanced and the anaerobic nature of the fundamental chemical changes in the cell still more definitely insisted upon by A. Gautier. His ideas are summed up in his admirable little *Chimie de la Cellule Vivante* (2nd edition, Paris, 1898). After describing his experiments, he concludes:

"Anaerobic life which up till then had been thought to be characteristic only of certain primitive micro-organisms, in particular of the schizomycetes, is the essential and fundamental mode in which most protoplasm functions. This view is founded on three orders of proof. First that the animal organism produces a number of reduced substances, among others the ptomaines and leucomaines, that I had just discovered; second, that in the humours of the economy are found the identical substances that I had shown are formed in the course of bacterial decomposition of the albumens, lactic acid, different fatty acids, amidatid compounds, phenols, carbonic acid and ammonia; third, that the quantity of oxygen found in the totality of our excretions exceeds by about one-fifth the quantity of oxygen taken from the inspired air" (p. 99).

Meanwhile, in 1875, Pflueger had repeated Spallanzani's experiments on frogs and confirmed the fact that when put into a

vacuum they continue to give off carbonic acid. A little later the same phenomenon was observed by Pfeffer in plants. Pflueger gave to this process the term "intra-molecular respiration." "So firmly had the conception of combustion been impressed upon physiologists that when this anaerobic respiration came to be explained, it was supposed that certain molecules of organic matter within the cell gave up their oxygen to others that they might thus be burned in the body furnace to yield energy."[1]

The name, however, was but a mask for mystery. Thanks to the work of the physiological botanists and especially to E. Godlewski, to W. Palladin and their respective schools, to Kostytschew, Stoklasa, Mazé and others,[2] the matter has now been cleared up. In the absence of oxygen, the respiration of plants we may surmise, as that of animals also, for the most part is a process strictly comparable with alcoholic fermentation. In order to exclude the intervention of any mystic "vital agencies" Palladin[3] conducted his experiments upon plants killed by freezing. But latterly Kostytschew[4] has been able to show a case of anaerobic respiration in the living plant in which there is no simultaneous production of alcohol. If *Agaricus campestris* be cultivated under aseptic and anaerobic conditions, no alcohol is formed; moreover an increased supply of sugar brought no increase in the production of carbon dioxide.[5]

An endeavour to identify respiration as essentially a process of hydroxylation has been made by A. P. Matthews and by Barnes. In his paper on "The Nature of Protoplasmic Respiration and Growth,"[6] Matthews notes, among others, the following facts which must be explained by any adequate theory:

1. "Even though surrounded by oxygen all protoplasm maintains itself while alive in a reduced state and acts as an intense reducing agent. Its reducing powers are comparable with those of nascent hydrogen.

[1] Barnes, "Theory of Respiration," *Bot. Gaz.* 81, **39**, 1905.

[2] Cf. Euler, *Pflanzenchemie*, p. 171 ff.

[3] *Bot. Ber.* v. **23**, **24**; *Zeit. f. physiol. Chem.* 407, **47**, 1906; *Biochem. Zeit.* 151, **18**, 1909.

[4] *Zeit. f. physiol. Chem.* 214, **48**, 1906.

[5] These observations have been confirmed by Stoklasa, among others, *ibid.* 303, **49**, 1907.

[6] *Biol. Bull. Woods Holl*, 331, **8**, 1905.

2. "For all forms of protoplasm oxygen above a certain tension is an intense poison. For some forms this tension may be no more than a very small fraction of an atmosphere; for others it is from three to four atmospheres.

3. "Atmospheric oxygen has little oxidising power; whereas protoplasm brings about oxidation of the most radical nature and is comparable in its oxidising powers to the most intense chemical oxidising agents.

4. "Hydrogen is evolved as a gas by a great variety of bacteria and moulds.

5. "Many forms of protoplasm such as the anaerobic bacteria are able to bring about intense oxidations in the absence of atmospheric oxygen."

Matthews adds:

"The studies of Armstrong, Dixon and others on the processes of slow and explosive oxidations and combustions throw, in my opinion, a remarkably clear light on protoplasmic respiration. These authors have shown that in ordinary oxidation the presence of some water is necessary to the oxidation. Phosphorus in a perfectly dry state will not ignite in dry air. Armstrong concludes that the primary oxidation in all these cases is not brought about by the gaseous oxygen but by the water. The atmospheric oxygen acts the part only of a depolariser to take care of the nascent hydrogen formed from the water.

"The same hypothesis will explain protoplasmic respiration and at once makes clear the identity of anaerobic and aerobic respiration. The following theory of respiration is founded in part on Armstrong's work:

"The real respiration of all forms of protoplasm, both aerobic and anaerobic, is brought about not by the oxygen of the air but by that of the water. The hydrogen set free from the water combines with other elements of the protoplasm, thus keeping it reduced; it also combines with the oxygen of the air if this is present to form water; and in the absence of oxygen it may escape as free hydrogen.

"The only difference between anaerobic and aerobic respiration is that the anaerobic protoplasm is so powerful a reducing agent that it is able to drive hydrogen out of the water, thus oxidising itself without the aid of atmospheric oxygen to act as a depolariser."

Primarily then, in Matthews' view, "respiration is the dissociation of water accompanied by the liberation of hydrogen."

Practically the same view is sustained by Barnes from the botanical side. In view of the fact that the word "respiration"

now connotes ideas no longer consistent with the facts, Barnes proposed the terms aerobic and anaerobic *energesis*, to which fermentative energesis might be added if necessary.

Still further proof that the characteristics of living protoplasm are those of a reducing agent have been brought forward by Loew and Bokorny[1] in their well-known endeavour to establish a chemical difference between the living proteins and "dead proteins." The active or living protein behaves as an aldehyde and displays marked reducing powers. This latter capacity is not, according to these authors, displayed by dead protein.

Finally as nearly conclusive proof that, in the plant at least, uncombined oxygen has little or nothing to do with respiration, it is to be noted that in plant respiration a direct relationship between the concentration of the oxygen and of the carbonic acid is directly demonstrable. In agreement with the law of mass action, a high concentration (four to eight per cent.) markedly lowers the respiration rate.[2]

On the other hand, any influence arising from a variation in the partial pressure in oxygen is extremely difficult to demonstrate. "Even in the higher plants," says Euler, "in which chemical differentiation has been carried to the farthest degree, a widespread indifference is to be found towards such a variation, so that in many species respiration continues without interruption even when the air contains only two per cent. of oxygen (according to Stich) or indeed only one per cent. (according to Johannsen). Equally indifferent are most plants towards a notable increase of the oxygen concentration, so that the latter may be increased by at least four or five times the normal amount without producing any effect. And as Bert has shown bacteria may endure simply enormous pressures" (Euler, *l.c.* p. 166).

III. *The Genetic Relationship of Anaerobic and Aerobic Life.*—If the simplest and most primitive of living organisms are anaerobic; if moreover the fundamental cellular metabolism be in all cases essentially anaerobic and oxidation and combustion, as it were, exterior or secondary processes; if finally, our faith in the reality of evolution be not somewhere lost in descending the scale of life, then it must follow that there is a genetic

[1] *Die chem. Ursache d. Lebens* (München, 1881); O. Loew, *Die chem. Energi d. lebenden Zellen*, 2te auf. (Stuttgart, 1906).

[2] Godlewski, *Arb. bot. Inst. Wurzburg*, v. 1, 1873.

relationship between anaerobic and aerobic respiration; that complete aerobiosis represents an evolution from complete anaerobiosis; further, that in all probability the middle forms of this evolution may be discovered.

Such a conclusion has already been foreshadowed in the preceding section; the idea was first put forward in tentative fashion by Pfeffer[1] as far back as 1878 but rather hesitatingly sustained in his further researches published in 1884–5.[2] Pfeffer's facts, however, did not seem entirely conclusive even to him; and it remained for Godlewski, Palladin and their followers to establish this relationship on a firm experimental basis. Part of this work has already been noted; the main facts which speak for the conclusion may be summed up as follows:

1. That between the forms which are strictly anaerobic and those which are strictly aerobic there are countless forms which are more or less indifferently one or the other.
2. That this latent capacity may apparently be developed in a wide number of forms which naturally appear to be decisively one or the other—that is, may be "adapted" to the one form of life or the other, by passage through a suitable number of cultures.
3. That aerobic respiration appears to be simply a heightened form of anaerobic respiration, carrying the chemical process one step further and ordinarily at a much more rapid rate.
4. That many life processes, as for example the germination of seeds, may be carried a certain distance in the absence of oxygen; they then come to a stop but the processes are resumed when either oxygen or an easily oxidisable substance like sugar is supplied.
5. That the establishment of the essential part played by oxygen alike in the plant and animal economy, viz. that of a waste remover, makes clear the natural relations of these two forms of respiration and the nature of the "evolution" from one form to the other.
6. That the need of oxygen is apparently directly related to the combustion or "metabolism" of fats and carbohydrates; that both of these processes are a function of temperature and that as temperature decreases the need of oxygen sinks rapidly, so that at very low temperatures (under 10° C.) metabolism is largely, if not exclusively of protein, *i.e.* anaerobic.
7. That the processes of fermentation are identical with

[1] W. Pfeffer, *Landw. Jahrb.* 806, **11**, 1878.
[2] *Unters. a.d. bot. Inst. zu Tübingen*, 105, **1**, 1884–5.

those of anaerobic life or a yet lower term in the same genetic relationship; and that these processes, *i.e.* hydrolyses and cleavages, are fundamental to all vital action whatsoever and everywhere antecedent to the intervention of oxygen.

8. That if oxidation were a fundamental process, life in the absence of oxygen or of any oxygen-supplying materials whatsoever would be inexplicable—*i.e.* that the "adaptation" of anaerobic organisms to an oxygen "habit" is conceivable and may be understood, whilst in the light of our present knowledge the reverse is not.

It is perfectly clear that the relationship between anaerobic and aerobic respiration (and fermentation as well) is of the closest and represents a *continuum.* Save between the extremes of these two terms there is no sense of antagonism; on the contrary, as Bunge[1] observes: "Apparently from the anaerobic single-celled organisms up to the most highly organised animal with the liveliest need of oxygen, we have in the animal kingdom every stage of transition." From being incessant, the need of oxygen dwindles away until it is entirely lost. At the opposite extreme we have an utter intolerance of oxygen, which in turn shades away into toleration; and this latter condition may be cultivated apparently in the most strenuously anaerobic types.

It remains then simply to establish whether the one type or the other is fundamental and if so which is the fundamental type. If any doubt remained, it would be destroyed by the fifth of the considerations noted above, viz. the establishment of the actual part played by uncombined oxygen in the chemistry of life. Amid a quantity of admirable work, that of Verworn and his school is among the most recent and most decisive. Summing up a long series of researches H. Winterstein[2] concludes:

"It must be considered that in the mechanism of tissue respiration energy is primarily derived from cleavage processes of a non-oxidative nature. The products so formed (the suffocation and fatigue toxins) are then as a secondary process oxidised by free oxygen. This oxidation may follow immediately on the cleavage; it may be separated therefrom both in point of time and place; or in the case of many organisms may be suppressed for a longer or shorter time (temporary or continued anaerobiosis)."

[1] *Zeit. f. physiol. Chem.* 565, **12**, 1888.

[2] "U. Mech. d. Gewebsatmung," *Zeit. f. allg. Physiol.* 392, **6**, 1907.

As a conclusion to his work, already cited, Pütter makes the following observations to a similar effect:

"As a result of comparative physiological studies of the significance of oxygen in life, we may now lay down the rule that, in general, respiration is a cleavage respiration and that in the absence of (free) oxygen the energy of the organism is supplied by hydrolytic changes. The utilisation of oxygen in functional metabolism, through which a much broader elimination of the respiratory materials is made possible, represents rather a specialisation; this, it is true, is very widely distributed and of very great practical importance but it should not lead us so greatly to overvalue it as to obscure the general rule, for the general rule is anaerobiosis" (*l.c.*).

From his work on the respiration of plants W. Palladin comes to the same conclusion. He says:

"As long as observers confined themselves to living plants, it was always possible to talk of 'adaptations' to special conditions, such as the artificial subtraction of oxygen and offer this as an objection to Pfeffer's theory. But it is difficult to imagine 'adaptability' in *dead* plants. If dead plants are capable of producing carbon dioxide after the withdrawal of oxygen, we must admit that the same interactions take place in life, in the presence of oxygen. On the basis of these researches on dead plants, it may be regarded as proven that the process of anaerobic cleavage is the primary process in respiration and that it is brought about by enzymes."[1]

Finally we have a series of decisive experiments by E. J. Lesser,[2] offering conclusive proof that "the production of carbonic acid by the frog in the absence of oxygen is not due to the presence of 'stored up' oxygen as was so long believed." In other words it seems clear enough from all this mass of work that free oxygen is not concerned in the fundamental chemical processes of the cell and that the sooner we get rid of this long-lived obsession the sooner we shall gain a clear insight into the nature of the primitive life process.

It is to be noted that in Winterstein's work special attention was paid to the relation of oxidation to the nervous system. If there be one form of tissue whose functional activity seems

[1] "Die Einheit d. Atmung," *Arch. f. d. Ausbau. d. Entw. lehre*, 203, **3**, 1909. Cf. *Biochem. Zeit.* 151, **18**, 1909.

[2] *Zeit. f. Biol.* 287, **51**, 1908.

absolutely dependent upon a supply of uncombined oxygen it is the nerve tissue, yet Winterstein's conclusion is that "a supply of free oxygen to the nervous system of the frog cannot be demonstrated" (*l.c.*).

The researches of J. Loeb tended in the same direction. Concerning mammals and birds, Loeb states: "The blood supply to the nerves is either lacking or is so meagre that we must conclude that the functions of the nerves require very little oxygen."[1]

On the other hand of the three main modes of animal nutrition, it is the deprivation of air or oxygen which brings death most swiftly. Unconsciousness comes more quickly still. Speck has shown that if the partial pressure of oxygen in the air is lowered to below one-third of its normal value, the fundament of mental activity, viz. the memory, is almost instantly interfered with and total loss of consciousness rapidly follows.[2] What then is the explanation?

A multitude of experiments have shown that many of the normal products of "metabolic activity," that is of the cellular chemism, are very poisonous, often to an extreme degree. Further that *all* true proteins, whether of bacterial, vegetable or animal origin, contain a highly toxic group.[3] The characteristic of these highly poisonous bodies is that they are in a reduced state[4] and apparently, in proportion as they are saturated with oxygen their toxicity is lost. This seems generally true of the poisons. To cite a familiar example, carbon monoxide is deadly in almost minimal quantities, whilst carbon dioxide may be supported in relatively large amounts—for example, ten times that normally present in the atmosphere affects the human organism but little.

It has further been shown that it is especially the products of anaerobic activity, *i.e.* the unsaturated vital products, which are notably toxic and that this toxicity is diminished or lost in the case of various bacteria when they are cultivated under the influence of light in the presence of oxygen or simply

[1] *Dynamics of Living Matter*, p. 18, 1906.

[2] Cited after Loeb, *l.c.* p. 6.

[3] Cf. the interesting researches of S. M. Wheeler, *Jour. of Biol. Chem.* 514, 6, 1909, with further ref.

[4] Cf. the researches of L. Liebermann, jun., *Cent. f. Bakt.* I, Bd. 51, Heft 4, September 1909, which go to show that all bacteria are strongly reducing substances.

exposed to the air.[1] Of all the products of the body tissues it is apparently from those which have the least supply of oxygen, the most nearly anaerobic, that is the nerves, that the most poisonous substances come, a fact which may explain the baffling and elusive nature of neurasthenia, the typical disease of persons who otherwise appear "well."

These general considerations have received almost conclusive demonstration from the researches of W. Weichardt.[2] Weichardt showed that when healthy rats were put on a tread-wheel and run until they dropped dead from exhaustion, the sap or juice expressed from their muscles was highly poisonous and produced all the effects of extreme fatigue and finally death when injected into the veins of other rats. The sap from control subjects showed no such toxicity.

Weichardt found that a much larger quantity of the fatigue-poison could be obtained when the exhausted muscles were treated with *reducing* substances, whilst the poisons disappeared spontaneously when the muscles were given time to rest. There seems little reason to doubt from all this that "fatigue" is simply accumulated poisoning and that "rest" is simply the oxidation of these *ermüdungs-toxine*. And this is likewise the *rationale* of the effects of "fresh" air. It is not the saturated products of respiration, like carbonic acid, which, as was so commonly believed, make us drowsy in a closed room, but the almost infinitesimal quantities of other substances which are given off from the lungs; it would seem even from the woodwork and other organic substances of a room.[3]

In the light of all this, the relaxations of anaerobic and aerobic respiration become very simple and clear. In the lowest and most minute of organisms the proportion of surface to volume is of course at the maximum and the number of individual living units interacting within the single cell is at the minimum. Some interesting researches of Errera[4] and of MacKendrick[5] indicate that the number of these living molecules in the simplest organisms may not be over a score or more and perhaps less.

[1] Kastle and Elvove, *Am. Chem. Jour.* 195, **31**, 1904; cited after Loeb, *l.c.*

[2] *Muenchener Med. Woch.* 1905, 1906, 1907.

[3] Henriet, "L'Atmosphère des Villes" and "Les Causes de l'Altération de l'Air Confiné," *Revue Générale des Sciences*, 183 and 483, **18**, 1907.

[4] *Rec. de l'Inst. Bot. Brux.* 73, **6**, 1903.

[5] *Rep. Brit. Assoc.* 1901, p. 808.

Obviously the chance of an interchange of their products with the surrounding medium, which is the very essence of life, will be far greater in the single-celled organisms than in the multicellular organism with its pipes and tubes. In the latter the chance for accumulation of by-products and therefore the need of a swift and effective neutraliser, like oxygen, will be correspondingly greater.

We seem to have here a simple picture why it is that the oxygen-need should increase with complexity of organisation. The differences shown by organisms, high and low, of about the same grade of organisation would appear to be due to those differences of chemical composition which present-day research, especially the "biological" or precipitin action, have so amply disclosed.

IV. *Anaerobiosis and the Biogenetic Law.*—We may now put the theory of anaerobic origins to what, in many minds at least, is a crucial test.

It seems to be fairly clear that every organism is in some sense an historical document [1] and that in a broad way the development of every individual (ontogeny) reflects the history of its race (phylogeny). This is the recapitulation [2] or "repetition" [3] theory or biogenetic "law," in Haeckel's phrase,[4] so long discussed, so much disputed, which now seems in process of being firmly grounded on bio-chemical experiment,[5] free from the subjective idiosyncrasies which have so obviously begotten the acrimonious controversies which this question has aroused, although it is evident enough that we cannot regard it as a "law" in any rigid sense.[6]

Likewise there are now few, even among biologists, who doubt that the processes of growth have a chemical basis [7] and

[1] Cf. Boveri's fine essay, *Die Organismen als Historische Wesen* (Würzburg, 1906).

[2] An admirable critical summary is Vailleton's *Un problème de l'Evolution* (Paris, 1908).

[3] T. H. Morgan, *Evolution and Adaptation*, p. 58 (N.Y. 1903).

[4] For an excellent though partisan review, cf. H. Schmidt, *Das Biogenetische Grundgesetz* (Frankfurt, 2te auf. 1909).

[5] E. Abderhalden, "Der Arten begriff," *Naturw. Runds.* 557, **19**, 1904.

[6] Cf. L. Plate, *Archiv f. Rassenbiol.* 198 and 721, **3**, 1906; Weismann, *Vorl. u. Descendenztheorien*, 179, II. 1902.

[7] F. Hofmeister, *Die Chem. Org. d. Zelle*, 1904; Hober, *Physik. Chem. d. Zelle*, Kp. 15; G. Ciamician, *La Chimica Organica negli Organismi* (Bologna, 1908).

that the structural and morphological differences between species, classes and orders are in the end the expression of definite chemical differences.[1] This is clear enough from various considerations: from the fact that the hæmoglobin of different species crystallises in different systems; from the production of a vast variety of distinct chemical substances, as for example, the organic acids, some of which are *characteristic* of a given species; and from many similar considerations but above all from the wondrous development of blood- and serum-chemistry, revealing as this does the most curious affinities, antagonisms, differences and degrees of differences among the different types.[2]

If this be true broadly, for racial characteristics, that is to say for evolution in general, it must be true in greater or less degree for each individual organism.[3] The latter must present a corresponding chemical evolution through the different stages of its life; and for this there is a considerable and constantly increasing evidence.[4] Just as, for example, certain of the vital enzymes appear only at certain definite stages of development, as the lab ferments in the second month of fœtal life in the human organism, diastase in the third month succeeding birth,[5] etc., so in the ripened fruit are certain chemical substances which are not to be found in the leaf nor the stem nor the root nor the seed.[6]

Upon all this we might base a reasonable expectation that if, in the evolutionary scale, we find an increasing need of oxygen corresponding to an increasing complexity of structural and chemical organisation; if in other words primitive life is anaerobic and ordinary aerobic respiration represents an evolution from this fundamental form; and if, further, the recapitulation theory holds good, we shall find all this mirrored in individual

[1] Hüppert, *Erhaltung der Arteigenschaften* (Prag, 1896); F. Hamburger, *Arteigens. und Assimilation* (Leipzig, 1903); A. Gautier, *Variation des Races et des Espèces* (Paris, 1901).

[2] G. H. Nuttall, *Blood Immunity and Blood Relationship* (London, 1904); Uhlenhuth, *Beiheft. z. Med. Klinik.* Heft 9, 1907; H. Friedenthal, *Arbeit aus exp. Physiol.* 1908.

[3] Cf. V. Ducceschi, *Evoluzione Morfologica ed Evoluzione Chimica* (Bologna, 1904).

[4] H. G. Wells, *Trans. Chicago Path. Soc.* May 1, 1909.

[5] F. Hamburger, *l.c.*; Oppenheimer, *Die Fermente*, 3te auf. Spec. Th. 1909.

[6] Cf. the researches of Bigelow and Gore, U.S. Dept. of Agric. Bureau of Chem. Bulls. Nos. 94, 97, 1905.

development. The embryo will have less need relatively of oxygen than the adult and this need will increase parallel with the growth of the organism. What now are the facts ?

W. Roux and Jacques Loeb were among the first to investigate this question. Roux found that the eggs investigated by him were incapable of development in the complete absence of oxygen and soon disintegrated. This result was confirmed in part by Loeb,[1] who, however, found that among the eggs of various animals there is a wide disparity in this respect. Thus for example, the eggs of surface-swimming animals are very sensitive to the absence of oxygen, whilst in those whose density is sufficient to cause them to sink the oxygen need is enormously reduced. So for example, the eggs of Fundulus continued to divide during twenty-four hours in an oxygen vacuum and during twelve hours in a current of hydrogen.

But what was more important still, Loeb established the fact that "the embryo is the more sensitive to a lack of oxygen the older it is." Thus an egg put into an oxygen vacuum twenty-four hours, thirty-six hours, etc., after fertilisation was correspondingly less resistent to this withdrawal. So in turn the newly-born fish was still less resistent than the embryo (*l.c.*).

This general law established by Loeb, that the need of oxygen is inversely proportional to age (*i.e.* size) has been further extended and confirmed through the researches of H. Samassa, E. Godlewski and K. Amerling for animals and by W. Palladin and O. Nobokich especially for plants.

Samassa[2] found and Godlewski[3] in a series of researches confirmed the fact, that frogs' eggs continued to divide during twenty hours after fertilisation in the complete absence of oxygen. Compare now this result with the experiments of Amerling[4] on the larvæ of frogs at different periods of development. The latter took as his test the time which elapsed before the larvæ showed distinct signs of cessation of their normal activities (Lähmung). His results are shown in the following table :

[1] "U. d. Empfindl. v. Fischemb. geg. Sauerstoffmangel in versch. entw. Stadien," *Pflueger's Archiv*, 530, **55**, 1894 ; "Unt. u. d. physiol. Wirkungen d. Sauersmangels," *ibid.* 249, **62**, 1896.

[2] *Verh. d. deut. Zool. Gesell.* 1896.

[3] *Archiv f. Entw.* 585, **11**, 1901 (for full literature).

[4] *Pflueger's Archiv*, 369, **121**, 1908.

Stage.	Body Length in mm.	Duration of Activity.
1	5	10 hours
2	6	7 „
3	7	8 „
4	8	6 „
5	11	2 „ 15 min.
6	15	1 hour 10 „
7	20	1 „ 0 „
8	24	55 „
9	32	52 „
10	34	40 „

In conclusion Amerling observes :

"Whilst these experiments have been conducted on those stages in which the membrane has been shed, it is obvious that from the beginning of division to the stage of metamorphosis, that is during the entire larval life of the frog, sensitiveness toward the withdrawal of oxygen progressively increases" (*l.c.*).

These results find a clear explanation and the theory here under review a solid support, in the researches of A. Herlitzka on the ontogenesis of the enzymes in the germinating organism and especially the date of the appearance of the oxidases. It is well known since the classic work of Bourquelot, G. Bertrand and others that the oxidations of the organism are effected exclusively by means of oxidising catalysts, the so-called oxidases. In the eggs of the viviparous animals and presumably in the unfertilised germ cells of the vivipares, these oxidases are apparently absent or in an inactive state. Says Herlitzka :[1]

"It is only at a late stage in the course of development that the oxidases and peroxidases appear. The poverty of oxidative processes which is indicated by this lack of oxidising ferments finds confirmation, in the case of frogs, in the fact that in the embryos hæmoglobin is for a long time lacking in the circulating blood. The same is true of the permanent larvæ of the Congeracei (teleosts). For the rest, such a limited development of the oxidative processes was to be foreseen in organisms which undergo such rapid growth."

The same general conditions appear to obtain among plants. Thus, for example, Godlewski[2] found that lupine seeds

[1] "Sur l'Ontogenèse des ferments," *Archives ital. de biol.* 119, **48**, 1907 (from *Biologica*, vol. i. No. 7, 1907), with full lit.

[2] *Bull. Acad. Soc. Cracovie*, 1904 : cited after Euler, *l.c.* 159.

germinate and show a vigorous development in the complete absence of oxygen when an adequate supply of sugar is present. O. Nobokich [1] showed that in Indian corn (*Zea Mays*) normal karyokinesis may continue during forty-three hours in the absence of oxygen.

Palladin sums up his researches thus:

"As to the capacity of plants in general to endure the withdrawal of oxygen (anaerobiosis) one may say—disregarding single instances of certain sharply defined cases—that the lower plants support anaerobiosis better than the higher and the embryonal or young stages better than the full grown." [2]

From this wide series of researches on the most diverse organisms of both the animal and vegetable kingdoms, it is evident that we have here, first of all, one of the most admirable and best sustained proofs of the fundamental validity of the recapitulation theory or biogenetic law, free from the personal bias of investigators; and secondly a broad curve of continuity, in individual development, parallel to the same curve of continuity which obtains in racial development. Alike in the single organism, as in differing types, the need of oxygen is the result of and increases proportionally with increasing complexity of organisation.

But apparently the simplest cell which we know is itself a highly complex symbiosis, colony or society of independent living units.[3] In no other way can we account for the characteristics and life activities of the granules or plastids which are seen growing and dividing in the living cell and whose division is apparently precedent to the division of the cell itself.

Let us now prolong this curve of decreasing oxygen need with decreasing complexity and it is apparent that we soon reach a point where this need approaches zero. Have we any right to assume that this curve is an asymtote? Clearly the

[1] Cited after Lesser, *l.c.*

[2] *Zeit. f. d. Ausb. d. Entw. lehre*, 206, **3**, 1809 (with full reference to previous papers).

[3] For literature cf. M. Heidenhain, *Plasma u. Zelle, IV. Abs.* (*die Granulalehre*), p. 327 ff. (Jena, 1907); R. Altmann, *D. Granulatheorie*, 2te auf. (Braunschweig, 1894); G. Schlater, *Zelle, Bioblast u. leb. Substanz* (St. Petersburg, 1903); Wilson, *The Cell in Development and Inheritance*, p. 22 (New York, 2nd ed. 1900); O. Hertwig, *Allg. Biol.* p. 26, 3te auf. (Jena, 1909).

demonstrated existence of absolutely anaerobic organisms is evidence that we have not.

Doubtless it would be a little hazardous to describe all metacellular organisms simply as congeries or colonies of anaerobic plastids, granules, biophores or "protomeres."[1] And yet many facts are in favour of such a conception. If it can be established and such is the present drift, we shall then have a clear, simple, mechanical explanation of what is now wrapped in fog.

In any event, this much is sure: at the foot of the life-scale, alike in the simplest of organisms and in the embryonic conditions of all organisms, the need of oxygen is wholly lacking or very slight.

This must have been true historically, if evolution be true; else we shall for ever remain without the remotest clue to the actual mode of the beginning of life, either in this or in any other regard, for alone in the fact or conception that the living world and each individual organism is, like the earth itself, a mirror of its own history, is any kind of clue to be found. We might manufacture life in the laboratory, as in the laboratory we may, for example, make coal. But in neither the one instance nor the other would there be any necessary or positive indication of their natural origin. Here biology and geology, biogeny and geogeny, are at one, for every direct trace of the early history of life, like that of the primitive history of the earth,[2] has been utterly and for ever destroyed.

Yet geology has its word on the special problem here under consideration. There is oxygen in the present atmosphere: was there any in eozoic time?

V. *The Atmosphere of the Primitive Earth.*—When we consider the oxygen of the atmosphere in present and in former times, we meet with the following facts:

In the lower levels of the existing atmosphere oxygen forms a little less than one-fourth of the air, by weight, the amount decreasing as we ascend, so that in the upper levels of the atmosphere there is no oxygen at all.[3] Even after including

[1] Heidenhain, *l.c.* p. 493.

[2] For a vivid presentation of this latter fact, cf. chaps. 11 and 14 of J. Walther's *Gesch. d. Erde* (Leipzig, 1908).

[3] Hann, *Lehr. d. Meteorologie*, p. 8, 2te auf. (Leipzig, 1906).

the amount dissolved in sea water, the quantity of uncombined oxygen as compared with the combined oxygen of water, for example, is almost infinitesimal. It is computed that the waters of the ocean, evenly distributed over the globe, would form a spherical shell about 2,500 metres thick.[1] This would represent an average pressure of 250 "atmospheres," and as eight-ninths, by weight, of this water is oxygen, it follows that this would be easily 1,000 or 1,200 times more than the uncombined oxygen contained in the atmosphere, the ocean, etc.

We have next the enormous quantity of oxygen locked up in the earth's crust. The average thickness of the sedimentary rocks exposed to view has been variously computed at from 10,000 to 50,000 metres.[2] Let us for the argument take the lower of these estimates; the relative density of the rocks is approximately 2·5 and they are about one-half composed of oxygen.[3] The total amount then is at least five times the amount in the sea, it may readily be ten or twenty times. It follows, therefore, that the amount of atmospheric oxygen is certainly not a $\frac{1}{5000}$, possibly not a $\frac{1}{25000}$ part, of the total amount in the outer envelope of the earth. As to the amount in the interior of the earth we of course know nothing whatever.

It is very well known that large quantities of oxygen are withdrawn from the air by the oxidation of the materials of the rocks, the respiration of animals, etc., and that there is practically but a single source of return supply, that of plant activity. We may then regard the oxygen in the existing atmosphere in one of three ways:

1. As the residue of an enormously greater quantity in former times.
2. As liberated under the influence of internal heat, volcanic eruptions and the like from that occluded in the rocks.
3. As due entirely to plant action.

1. So far as the first supposition is concerned, we are met by the fact that throughout the entire lithosphere or rocky crust are masses of oxidisable but unoxidised materials, large areas of which must have been exposed at one time or another to atmospheric action. This fact, familiar since the days of

[1] Richthofen, *Das Meer* (Berlin, 1904).
[2] Geikie, *Geol.* 3rd ed. (London, 1904).
[3] Clarke, *Data of Geochemistry*, p. 18 (Washington, 1908).

Daubeny,[1] is not compatible with the view of a previous great excess of oxygen.

Further than this, it would seem that in former geologic times, rock weathering and oxidation must have gone on rather more intensely and on a wider scale than at present. It is now increasingly evident that in the earlier ages at least the land areas were devoid of vegetation.[2]

The continents appear to have been utter deserts, such as are now unknown. We have clear evidence of the existence of an atmosphere and likewise of climatic zones,[3] much as at the present day; the average temperature was probably much the same as now.[4] These desert conditions would seem to indicate much greater and more frequent precipitation and far more violent winds and storms. Æolian erosion would have been far more prevalent and active than now and the plantless continents would have been swept by enormous dust storms. Under these conditions, oxidation would have gone on at a very intense rate; so that if, as some writers suppose, a greater excess of oxygen existed in the beginning, this would have been quickly and completely exhausted. We may therefore dismiss the first possibility.

2. Within the last few years, thanks to the researches of Moissan,[5] Gautier[6] and A. Brun,[7] we have learned something of volcanic chemistry and it may be taken that practically no uncombined oxygen is present in volcanic emanations. On the other hand, we know of vast quantities of hydrogen compounds and there is the probability of free hydrogen, so that we have here an agency working for depletion of oxygen rather than the reverse. And plutonic action is probably only volcanic action on a very grand scale. For the rest there seems to be no oxygen free in meteors[8]; and there is no other visible source of supply. We may therefore dismiss the second possibility.

[1] Cf. his essay on the *Geological and Chemical Phenomena of Volcanoes* (Oxford, 1824).

[2] Walther, *Geschichte d. Erde*, 194 and 254 (Leipzig, 1908).

[3] Eckardt, *Das Klimaproblem*, p. 9 (Braunschweig, 1909).

[4] J. W. Gregory, *Climatic Variations* (Mexico City, 1906).

[5] *Proc. Roy. Soc.* 166, **60**, 1896.

[6] *Annales des Mines*, 316, **9**, 1906; *Revue Scient.* 545, 577, 1907.

[7] A. Brun, *Arch. d. Sciences de Genève*, 439, **19**, 1905; 425, **22**, 1906; February and July 1909.

[8] Clarke, *l.c.* p. 52.

3. It is a curious fact that we should be led to the third conclusion directly from consideration of a very simple fact. Taking the amount of coal estimated to be under the best known areas, Great Britain for example,[1] if we suppose that this represents the average distribution for all the continental and "transitional" (*i.e.* shallow water) levels of the earth, we arrive at a quantity of carbon which would be approximately the chemical equivalent of the amount of oxygen now in the atmosphere. We know that coal is the product of the great water-plants[2] which flourished at a maximum in the so-called Carboniferous period; in other words, it was due to the splitting up of carbon dioxide under leaf action. And it is supposed, as other considerations suggest, that in subsequent times the give and take of oxygen to and from the atmosphere has been maintained at something like the present balance.[3] Since we know of no other adequate source of oxygen, it seems fair to conclude, therefore, that antecedent to plant life the atmosphere was practically devoid of this element.

This view of an oxygenless atmosphere in former times was put forth as early as 1845 by C. J. Koene of Brussels. It has been supported in later times chiefly by T. L. Phipson,[4] J. Lemberg,[5] John Stevenson,[6] Lord Kelvin[7] and Arrhenius[8]; it would seem to be a necessary postulate of any meteoritic theory of earth origins, since, as noted, gaseous oxygen does not appear to be a constituent of meteorites. A radically opposite view, of a former enormous excess of oxygen, developed among others by H. Lenicque,[9] is due to exclusive consideration of the chemical side of the problem, without regard to the known geological facts. It is incompatible with the existence of oxidisable but unoxidised compounds, present in enormous quantities, especially and particularly in the igneous rocks. As already indicated, this would involve a very rapid absorption of oxygen

[1] Cf. the ingenious calculations of John Stevenson, *Phil. Mag.* 312, **50**, 1900; also Bischof, *Verh. d. niederrhein. Ges. f. Naturk.* 4 marz 1887.

[2] Walther, *l.c.* 306 ff.

[3] Gregory, *l.c.* with citations of literature; Eckardt, *l.c.* p. 8 ff.

[4] *Chem. News*, London, vols. 67–9, 1893–4.

[5] Cited after Clarke, *l.c.* p. 50.

[6] *l.c.*

[7] *Phil. Mag.* 66, **47**, 1899.

[8] *Werden d. Welten*, p. 50 (Leipzig, 1907); also in English translation.

[9] *Mem. Soc. ingén. civils France*, October 1903, p. 346.

as soon as magmatic extrusions were exposed to atmospheric action.

These considerations establish with a fair degree of probability, first that the oxygen in the present atmosphere has been derived largely if not exclusively from plant action; secondly that life began in the presence of very little oxygen, if the latter was not absolutely lacking. This conclusion derives further support from the history of the mechanism by which, in the above view, the oxygen we breathe has been set free.

It is well known that the characteristic plant function, the splitting up of carbon dioxide accompanied by the liberation of oxygen, is due to the presence of a complex substance, chlorophyll, whose function apparently is to absorb the lower rays of the spectrum and with this energy to dissociate the molecules of carbon dioxide and water;[1] and these according to the prevailing theory give rise to formaldehyde, CH_2O, oxygen being liberated.[2] These "colour bodies" or chloroplasts present one of the most interesting instances of transition from highly organised to living substance. There is no evidence, as yet, that they can live by themselves; on the other hand they divide and reproduce in exactly the same manner as unicellular animals and plants.[3]

We seem to have here the clearest evidence of a symbiosis; community-life, we know, extends very far down in the scale[4] even to the green algæ and their like. A step lower in the scale and this association with the complicated chlorophyll mechanism is lost. We have then the series of bacteria and moulds which, apparently, do not utilise carbonic acid and often give off hydrogen instead of oxygen.

It is to be noted further that in the metaphyta and most strikingly in the higher plants, the chlorophyll function does not exist obviously in the seed and does not appear until long after germination has begun. We should infer from this that the chlorophyll "habit" was of relatively late appearance in the plant world and this conclusion is precisely that to which we are led from consideration of the non-chlorophyllian autotropic

[1] G. Ciamician, *La Chimica Organica negli Organismi*, p. 14 (Bologna, 1908).

[2] Cf. Euler, *Pflanzenchemie*, 109 ff.; Maze, *L'Evolution du Carbone et de l'Azote* (Paris, 1899); Bokorny, *Naturw. Woch.* 361, **8**, 1909.

[3] O. Hertwig, *Allg. Biol.* 3te auf. p. 95 (Jena, 1909); J. R. Green, *Man. of Bot.* 3rd ed. p. 277 (London, 1904).

[4] P. Kammerer, *Archiv f. Rassenbiol.* 585, **6**, 1909.

organisms. In other words we have no reason whatever for supposing that the chlorophyllian plants are anywhere near the bottom of the life scale.[1]

The most reasonable supposition that we can make is that the liberation of oxygen in great quantities by plants began long after a teeming life filled the seas and had at least begun the conquest (*i.e.* occupation) of the soil. We have no reason, however, to suppose that the great bulk of the oxygen of the atmosphere was due to land plants. On the contrary, we have the clearest evidence, from the characteristic "colour zones,"[2] that climatic conditions were not essentially different far back in the beginnings of the Cambrian and probably antecedent to the Algonkian. It is possible that the chief producers of atmospheric oxygen were the green algæ of the sea and they may be still. Phipson[3] reports that the green algæ produce, weight for weight, fifty times the quantity of oxygen liberated from a common phanerogam, Polygonum.

To sum up: from every consideration affording a clue to the conditions under which life might have arisen naturally on the earth; from the study of the comparative biology of plant and animal and the protista as well as the embryological, developmental, physiological and bio-chemical processes; finally from such geological evidence as we possess, the one conclusion emerges: that the beginnings of life did not involve the intervention of uncombined oxygen.

The gain from this new point of view may not be great but it enables us to *delimit* the problem one step further, so that we may now say, in the light of our present knowledge: that since anaerobiosis and the fermentative processes are identical, the problem of the genesis of terrestrial life has become that of the natural origin of the organic fermentative substances or enzymes, whose simple collocation into plastids, protomeres, biophores or granules constitutes the primary living units. The aggregation of these latter in turn constitutes the "cell."

[1] For an interesting discussion of this point, cf. M. Kuckuck, *Problem d. Urzeugung*. p. 60 ff. (Leipzig, 1907).

[2] Walther, *l.c.* p. 198.

[3] *Chem. News*, vols. 67 and 68, 1893.

THE SOIL AND THE PLANT

A REVIEW OF SOME RECENT AMERICAN HYPOTHESES

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In order that the reader may be in a position to appreciate the exact force of the hypotheses to be discussed here and to understand the precise bearing of the points at issue in the controversies to which they have given rise, it is desirable at the outset to state briefly the salient features of the relationships between soil and plant.

Various conditions have to be fulfilled in order that a plant may make satisfactory growth: there must be sufficient food, water, air, warmth and light, also there must be an absence of harmful and inhibiting factors. Further, if the plant is to be grown economically and without undue risk of loss, the soil must provide a sufficiently firm anchorage ground, so that the plant can stand up well and not suffer too much from wind. For our present purpose, we may dismiss the action of light as being unconnected with the soil and group the remaining requirements in two main classes: food supply and certain inhibiting factors, which are largely connected with the chemical properties of the soil, form the one; the water, air, temperature and root-hold factors, which are more closely related to its physical properties, the second. The distinction is further convenient in that it also indicates the lines of cleavage in the discussions that have centred round the hypotheses now under discussion.

All these factors are of equal importance. A deficiency of any one of them sets a limit to the effectiveness of the others and consequently to the crop growth. Thus, no matter how much food may be supplied, the plant cannot make a corresponding growth if the water supply be insufficient or the temperature too low. It is therefore fatal to our subject to take

too narrow a view of the interrelationship of the plant and the soil; however necessary it may be for purposes of investigation to concentrate attention on one special factor or group of factors, any division along conventional biological, chemical or physical lines is almost certain, sooner or later, to lead to disaster, because of the risk of overlooking some other factor, outside the purview of the experimenter, that is limiting the effect of the particular factors under investigation.

Some nine or ten elements of value as nutritive materials, including nitrogen, phosphorus, potassium, sodium, calcium, magnesium, iron and others, are supplied to the plant by the soil. Long experience has shown, however, that addition to the soil of compounds of some of these elements, particularly of nitrates, phosphates, potassium salts and calcium carbonate, commonly leads to an increase of plant growth. Organic matter in the form of plant residues, dung, etc., also increases the crop. All the evidence so far available goes to show that the plant food is taken up from the solution made by the carbonic acid generated by micro-organisms or by plant roots. Thus the soil moisture functions in two capacities: it is the source of water for the plant; it is also the medium through which the food is absorbed.

Numerous investigations of the soil water have therefore been made. Its ultimate source of origin is the rain-water falling on the soil: part is held by surface attraction to the particles and part slowly percolates through the soil and runs away into the subsoil; the rest evaporates. A soil will never drain itself dry but always contains a certain amount of moisture, no matter how long the interval since the last rain. Indeed considerations of surface actions would lead us to expect a movement of water from the moister to the drier parts of the soil or, more strictly, from the places where the moisture films have a flat to those where they have a sharp curvature. How far this capillary movement actually takes place has not been determined. Evaporation is always going on and reducing the thickness of the films but it diminishes in amount very rapidly below the surface of the soil. The minimum amount of soil moisture not further reducible by drainage but only by evaporation depends on the size of the soil particles, their arrangement and the amount of organic matter present. As illustrations the following figures show the lowest amounts

of water found in certain soils periodically examined by the writer:

LOWEST AMOUNT OF WATER FOUND IN CERTAIN SOILS AFTER PERIODS OF DROUGHT, PER CENT. BY WEIGHT

Clay soil, Rothamsted.		Sandy soil, Millbrook.	Heavy clay soil, Woburn.
No organic manures supplied.	Annual dressings of farmyard manure.		
11·9	18·0	7·6	20·0

The air relationships are bound up with the water content of the soil. Only a limited amount of space is available in the soil for air and water. In some of the above cases the figures are:

VOLUME AVAILABLE FOR AIR AND WATER IN 100 VOLUMES OF SOIL IN—

	Rothamsted. No organic manure.	Rothamsted. Farmyard manure.
Total space unoccupied by soil particles . .	34	38
Minimum of water always present	23	30
Difference, being maximum volume of air that can be present	11	8

The volume of air present in the soil fluctuates from the maximum figures given above to the very small volume found during and shortly after heavy rain. Oxygen is continuously being absorbed and carbon dioxide evolved but usually atmospheric oxygen diffuses sufficiently rapidly into the soil to maintain there a partial pressure approximately equal to that outside. Thus the difference between the soil air and atmospheric air is that the former contains a few more volumes of carbon dioxide in 10,000 parts than the latter.

Injurious and inhibiting factors are of various kinds and form a highly vague group but some of them must be put out of action by calcium carbonate, because of the striking effect it has on soil fertility. Indeed from the vegetation standpoint, soils can be divided into two main groups—those that contain calcium carbonate and those that do not. So great is the distinction that the practical man has long since made use of a separate name for the latter soils and calls them "sour," a word which many writers naturally but not altogether correctly interpret as "acid."

These then are the main relationships which agricultural chemists are called upon to investigate. Of the various hypotheses as to the constitution of the soil that have been framed

during the course of these investigations, the most complete are those of Whitney, Cameron and others associated with the Bureau of Soils of the United States Department of Agriculture which we shall now proceed to set out.

The Hypotheses of the United States Bureau of Soils

The foundation of these hypotheses is the recognition of the vitally important functions of the soil moisture. The films of water surrounding the particles are looked upon as the nutrient solution feeding the plant and constituting by far the most important part of the soil. This soil solution has been studied in two respects—in relation to the plant and in relation to the soil; with remarkable and unexpected results in both cases. The relationship between the soil solution and the plant has been elucidated by a study of plant growth in various culture solutions. Perhaps the most striking conclusion is that the rate of plant growth is relatively constant over wide variations in the concentration of the solution; in Breazeale's experiments [5][1] an increase in the strength of the nutrient solution from 75 parts to 750 parts per million of total soluble salts produced only little effect on the growth of wheat seedlings. Variations in the relative proportions of the constituent salts have a much more marked effect and not only lead to changes in the rate of growth but also to changes in the character of the plant—*i.e.* the proportion of root to leaf, etc.

Perhaps the most important property of the solution in relation to the soil is that its concentration is practically constant in all ordinary soils. Taylor and Mooney (v. [28]) concluded from a large number of analyses that the amount of solution associated with a million parts of soil contained $PO_4 = 7{\cdot}6$, $NO_3 = 5{\cdot}5$, $Ca = 11{\cdot}7$, $K = 22{\cdot}7$. Whitney and Cameron [28] accept these figures which, they consider, "represent closely the average figures for the great majority of cultivable soils as regards these several constituents." It is since admitted that the nitrate figures are subject to considerable fluctuations and indeed that nitrates ought hardly to be regarded as mineral but as organic constituents of the solution, as they arise from

[1] The numbers in square brackets refer to the bibliography at the end of the paper.

organic matter; but the position is still maintained with regard to the phosphoric acid, potassium, and calcium.

The constancy in composition of the soil solution is readily accounted for. Most soils arise from a relatively small number of minerals. It is supposed that the process is mainly one of disintegration and not decomposition. The large fragments of the soil are shown to consist of unaltered minerals and the small fragments could probably all be identified if suitable methods were available. Thus most soils are chemically alike from the qualitative point of view; they are mixtures of the same complex minerals and differ only in the proportions of the mixture and the size of the fragments. In consequence the soil solution in equilibrium with these particles has the same composition in all cases. Further, it is argued that the addition of a soluble potassium salt to the soil cannot increase the concentration of the solution in K ions, since any of the new ions going into solution would simply throw others out of solution. The solubility product, it is supposed, must remain approximately constant whatever soluble salts are added.

The mineral particles of the soil constitute the reserve from which the soil solution is reinforced as fast as soluble material passes into the plant. But they serve another purpose; they form the great framework over which the soil solution arranges itself in accordance with the laws governing surface attractions. Any local disturbance brought about by the action of plants or other causes is readjusted at a speed which depends on the size and nature of the particles, the presence of organic matter, and various other factors. Whilst therefore all soils are alike in regard to the composition of the soil solution they show great differences in respect to the arrangement of the solution over their particles and the speed of readjustment; in other words, in the speed at which the nutrient solvent is supplied to the plant.

The fertility of the soil does not depend on the chemical composition of the soil for two very good reasons: the composition of the soil solution is the same in all soils; and even if it were not variations in composition would be without effect on plant growth. In most soils fertility depends on the physical factors regulating the supply of the soil solution to the plant.

But this simple view of the constitution of the soil must

in many cases be complicated by another factor because the relative infertility of a soil is often communicated to its aqueous extract. Thus two soils of Cecil clay, very similar in physical characteristics, show marked differences in fertility, and their aqueous extracts showed like differences. But we have seen that the soil solution is similar in composition in all soils and direct analysis proved these two aqueous extracts to be no exceptions to the rule. Hence the infertility of the poor soil was not due to any deficiency of food-stuffs, but to some toxic substance. Numerous other cases have been investigated, and wherever the relative infertility of a soil is transmitted to the aqueous extract it is supposed that some toxin is present. It is further considered that the toxin is organic.

So great was the toxicity of the solution that in some instances it was an even poorer medium for plant growth than distilled water. But Livingstone found [14] that its toxicity could be lessened in various ways: by dilution, when indeed it might actually increase plant growth; and also by shaking with calcium carbonate, precipitated ferric or aluminium hydroxides, cotton wool, shredded filter paper or carbon black; the solution then became much improved as a culture medium. Further, the addition of fertilisers, especially such as were known to benefit the soil, also improved the solution: the organic matter of dung and of green manure was very useful and had some specific effect not shown by the inorganic constituents. In one experiment wheat plants gave the following results:

	Amount of Transpiration.	Weight of plants, tops only. Green.	Dry.
I. Plants grown in soil extract alone . .	100	100	100
II. „ „ „ + dung extract . .	400	213	151
III. „ „ „ + ash of dung extract + $NaNO_3$ equivalent to nitrogen lost in preparing the ash	196	112	109

The value of the dung extract is clearly not due to its ash ingredients or its nitrogen, because all these are present in Series III. "That the organic matter is directly of use to plants as nutrient material is not probable," say the authors; "it appears to be beneficial largely through some action on the soil constituents." It is therefore considered that the infertility of many soils is due to toxic constituents, and as these are probably organic a double set of experiments was begun by Schreiner and others [16–21]; the effect on plant growth

of organic compounds likely to occur in the soil was studied by elaborate water cultures; and a careful search was made in the soil itself for such organic compounds as could be identified.

The water culture experiments showed that numerous substances, particularly neurine and guanidine, were toxic to plants. In general, oxidation reduced toxicity: choline was less and betain still less toxic than neurine. Tyrosin, again, was harmful but the black oxidation product it affords was actually beneficial. The toxicity of neurine and guanidine and presumably of the other substances also was reduced by addition of charcoal, pyrogallol, calcium carbonate and sodium nitrate. Further, when two crops were grown successively in the same solution the second did better than the first, an effect that may be connected with an oxidising power possessed by plant roots [16]. The combined effect of plant roots and of fertiliser was very potent in overcoming the effects of a toxic body.

The search for organic compounds in the soil is complicated by the presence of large amounts of sand, silt, clay and highly complex organic substances. In consequence the problem has not attracted workers; the first serious investigation was made by Schreiner and Shorey [19], who obtained a number of compounds, including two hydroxystearic acids, a piccoline carboxylic acid and several products of protein and nucleoprotein hydrolysis. The harmful effects on plant growth of one of these —dihydroxystearic acid—was investigated at some length [21].

Several attempts have been made to discover how these toxic organic substances arise in the soil. Whilst some are supposed to be normal decomposition products of the complex organic matter of the soil, others are considered to be excretions from plants. The old hypothesis of de Candolle is revived and these excretions are supposed to be harmful to other plants of the same kind but not necessarily to plants of a different kind. An explanation is thus afforded of what is said to be a common phenomenon: the sterility finally induced when one and the same crop is grown continuously on the same ground. Water culture experiments are quoted to show that wheat seedlings exude from their roots something that makes the solution toxic to a subsequent batch of seedlings.

Lastly, the part played by fertilisers has been studied. The beneficial effects of fertilisers are not in dispute but it is con-

tended that their action is much more complex than has hitherto been suspected. Most writers have maintained that fertilisers are simply foods; Whitney and his colleagues, on the other hand, consider their nutritive effect as relatively insignificant, or at least as only one of several beneficent functions. As the soil solution has the same composition in all cases there clearly is no exhaustion of food constituents from the soil, and "exhaustion" or "poverty" must therefore be explained in some other way. Great stress is laid on two functions, the physical effect of fertilisers on the distribution of the soil solution; and their influence in removing toxins or, as Whitney puts it, in soil sanitation.

The Points at Issue

We can summarise the hypotheses under seven headings:

(1) The soil is formed by disintegration of the rock-forming minerals, decomposition taking place to a relatively insignificant extent.

(2) All normal soils are therefore chemically alike and the soil solution has the same composition in all soils.

(3) The soil solution forms the nutrient solution for plants; all soils are therefore equally well provided with plant food.

(4) This nutrient solution is distributed over the surface of the soil particles in accordance with the laws governing surface attractions. The rate at which any disturbances are readjusted depends on certain properties of the particles and is of prime importance in determining the productiveness of soils.

(5) But in many cases there must be some other factor involved, because the aqueous extracts of soils, instead of behaving all alike as they should according to (2), often show the same differences in productiveness as the soils themselves. A toxin must therefore be present in infertile soils.

(6) Some toxins may arise normally during the decomposition of the soil organic matter but some are probably excreted by plants.

(7) Fertilisers do not function primarily in the soil as nutrients for the plants but they have a much more complex action. They may alter the distribution of the soil solution or throw out of action some of the toxins and probably they serve other functions also.

Discussion first centred round the relative importance of

the physical and chemical properties of soils and more recently around the alleged excretion of toxins by plants. That the physical properties of soil were of fundamental significance in determining fertility was recognised by all the older agricultural chemists although actual investigations were made only by Schübeler [22], Wollny [30], and a few others. It was not till Whitney's first papers appeared [24, 25] that the relationships became clear and definite; and when, later on, Briggs [6] and King [13] published their notable researches on the movements of water in soils, chemists were put in possession of useful working hypotheses and learnt how to connect up the properties of a soil with the properties of its component particles. It was shown that the size of the particles and the extent of their surfaces were highly important factors in regulating the water and air relationships of soils; further, that the known agricultural properties of the soil could be correlated with the amounts of the variously sized particles it contained, amounts readily determined by a mechanical analysis. These ideas were introduced into this country by Warington [23] and have so commended themselves that mechanical analysis is now a recognised part of the work of a soil chemist and indeed gives a better basis for explaining the agricultural properties than does a chemical analysis [11].

But we are not prepared to agree that the chemical properties of soils are relatively insignificant in determining fertility. If the coarser fractions of a soil are analysed they are found to be almost entirely silica, but the finer portions—below 0·005 mm. diameter—which play a controlling part in soil fertility are more complex and contain besides the undecomposed rock material a quantity of weathered silicates and decomposition products. The special properties of the finer material are associated by Whitney with their small dimensions, approximating in some cases to molecular dimensions, and not with any particular chemical compositions; but evidence is accumulating that mere smallness of size will not account for all the observed properties. The view most commonly held by chemists is due to van Bemmelen [1–4]. Under the conditions obtaining in the soil a number of the decomposition products are deposited in a colloidal form, hence the soil particles are coated with a colloidal complex containing silica, aluminium and ferric hydroxides, alkaline bases, phosphoric acid and humus, not in true chemical combination

or even in any simple proportion. This complex is decomposable by change in temperature, concentration, etc.; but the process is continuous and does not show the *per saltem* character of chemical change; the complex can be acted upon by various solutions (as for example added fertilisers), absorbing certain substances as a whole, *e.g.* organic substances, or simply giving up to the solution an amount of base equivalent to what it has absorbed. These changes, he has shown, follow the ordinary laws of absorption of colloids [2, 3]. On this view the concentration of the soil solution directly depends on the composition of the colloidal complex and may be altered within limits by addition of soluble salts.

Thus we are up against a question of fact—whether the soil solution is or is not constant in composition in the case of different soils. Unfortunately the difficulty of extracting the soil solution has not been satisfactorily overcome and recourse must be had to devices which give its composition only approximately. If we suppose that the water of a well represents the soil solution then there is clearly no sort of constancy of composition in passing from soil to soil, as can be seen by looking through the numerous recorded analyses. If we suppose that the drainage water from the top layer of soil, which is rather different in composition from well water, more nearly represents the soil solution we again find no evidence of constancy, although in the case of a particular field the drainage water may show but little variation throughout the whole season.[1] Unfortunately sufficient data are not yet collected. But perhaps it is fairest in this discussion to accept Whitney's method and to analyse the figures obtained by Taylor and Mooney on which he based his statements. Considerable variations are found; the highest and lowest values are:

	Highest.	Lowest.
Phosphoric acid	40·60	0·59
Nitric acid	62·00	trace
Calcium	102·85	trace
Potassium	62·20	4·93

[1] Von Seelhorst (*Journal für Landwirtschaft* 1901, 49, 251) made complete analyses, at first each week and afterwards each fortnight, of drainage water running from a field during the year in August 1899 to August 1900. The results in parts per million are:

	CaO	MgO	SO_3	K_2O	N_2O_5
Highest figures	184	46·4	59·2	3·7	8·2
Lowest figures	157	31·3	43·5	1·7	1·9

If we group the figures to show their distribution we find in the case of phosphoric acid the number of instances where there are :

Less than 5	parts per million	of dry soil	=	120	or 27·97	per cent.
5 to 10	„	„	=	193	„ 44·99	„
10 „ 20	„	„	=	105	„ 24·47	„
20 „ 30	„	„	=	8	„ 1·86	„
30 „ 40	„	„	=	3	„ 0·70	„

A similar distribution can be worked out for the other constituents. Under the circumstances we cannot accept the conclusion that the soil solution is of the same composition in all cases. The difference in view is more fundamental than it appears. Whitney tacitly assumes that the concentration of the soil solution is uniform throughout the mass of soil. On our view the concentration is subject to considerable local fluctuations, depending on the local partial pressure of carbon dioxide in the soil atmosphere which in turn depends on the distribution of roots and other living agencies, on the distribution of calcium carbonate, of water and other factors. Even if a quantity of the soil solution could be extracted we cannot agree that the average composition it possessed would have any real counterpart in the soil.

However, these criticisms are met by the argument that variations in concentration of the culture solution do not in any case influence the rate of plant growth and consequently any variations in the strength of the soil solution would be without effect on fertility. Again we cannot accept the statement. In an infinitely dilute nutrient solution, *i.e.* in pure distilled water, plants make no growth; the amount of growth increases with the concentration till some limiting factor intervenes, when further increase in concentration produces no increase in crop; finally, if the concentration become too great, secondary adverse effects set in and growth is depressed. There is a fairly wide range over which the limiting factor controls the situation and the nutrient solution may increase in composition without increasing plant growth; accordingly we find a number of culture solutions in use for plant physiological purposes, but we have no evidence whatever that the soil solution is anywhere near this range of concentration. The fact that plant nutrients such as nitrates, phosphates and potassium salts are among the commonest and most effective

manures, whilst non-nutrient salts are not in use for this pupose, is strong ground for allowing nutritive functions to fertilisers; nor, since their complex action is admitted, do we see what is gained by denying them such functions.

Among the numerous organic compounds of the soil that Schreiner and his co-workers are isolating, it is not surprising that some possess toxic properties. But it does not follow that a substance toxic in water culture is toxic in the soil. Powdered charcoal reduces or removes the toxicity of the solution apparently by absorbing the toxic substances; soil also is known to possess similar absorptive powers, indeed in one of Livingstone's experiments soil was actually found to reduce the toxicity. We cannot therefore take it as proved that toxic substances isolated from the soil have been acting as toxins. There is, however, indirect evidence of the presence of toxic substances in the soil under certain conditions. It has already been stated that soils can be divided into two classes in their relationship to plant growth according as they do or do not contain calcium carbonate. Soils deficient in calcium carbonate are known to be relatively infertile; the practical man calls them "sour" and writers usually explain matters by saying that acids are present which plants will not tolerate. There is reason to suppose that the micro-organic flora of the soil is altered when the calcium carbonate falls below a certain limit and we can take it as certain that the course of decomposition in such soils differs from that going on in presence of sufficient calcium carbonate. Unfortunately no investigations have been made in this country of "sour" soils in spite of the interesting problems they present, but there are indications pointing to the presence in them of toxic substances.

In their disregard for chemical analyses, the Bureau of Soils do not generally state whether or not the soils under investigation are rich in calcium carbonate. But from their general description it appears that the soils giving the toxic extracts are in the "sour" category, especially as calcium carbonate or lime commonly improve the soil and the extract. So far as "sour" soils are concerned, therefore, we agree with Whitney that toxic organic substances may be a cause of infertility. We have no evidence, however, that such substances are present in soils sufficiently provided with calcium carbonate, in which class are included all our well-farmed soils.

When we turn to the question of plant excretions we can find no evidence whatsoever in favour of their existence. At Rothamsted plants have been grown continuously in water cultures, in sand cultures and in soils without showing any sign of suffering from excretions of previous generations. Six crops of rye were grown in succession in sand to which only nutrient salts were added so as to maintain the food material at a constant amount. A seventh crop was then taken and at the same time a crop was grown on perfectly fresh sand on which nothing had grown before, supplied with an equal amount of the same nutrient salts. There was no significant difference in the two crop yields, they agreed within the 5 per cent. experimental error. A similar experiment was made with buck-wheat, another with spinach, and a parallel series was made in soil cultures. In all cases the result was the same; the 1910 weights were as follows:

1910 Crops: Mean of Four Pots

	Sand cultures.			Soil cultures.		
	Weight of dry matter, grams.			Weight of dry matter, grams.		
	Rye.	Buckwheat.	Spinach.	Rye.	Buckwheat.	Spinach.
Cropped six times previously	30·4	5·4	33·3	26·4	23·9	20·0
Fresh sand	31·3	13·5	29·5	27·1	25·2	20·8

Both sand and soil contained 2 per cent. of calcium carbonate.

If either the rye, buckwheat or spinach excreted any toxin the amount accumulating during the growth of six successive crops was insufficient to cause any appreciable depression in yield in the crop; the exceptional result given by buckwheat in sand could not be confirmed.

Thus we are once more up against a question of fact: it is asserted by the American workers that the continued growth of one and the same crop on the same soil leads to a low crop-production, whilst we on this side are unable to obtain any evidence to this effect.

But again the experimental difficulties are considerably greater than they appear at first sight. Water cultures do not long remain sterile but soon contain a bacterial population which clearly produces some change in the solution, because the nitrates quickly disappear. Sand cultures rapidly develop a vigorous growth of algæ and bacteria; here also nitrates

tend to disappear. Soil, we know, is the seat of many far-reaching changes. Thus we have never got a perfectly clear issue: the experiment has always been complicated by other changes taking place simultaneously. Our conclusion, therefore, is that there is no unexceptionable evidence of any toxic excretion by plant roots.

Many statements may be found in agricultural literature to the effect that certain crops poison others, or even young plants of the same kind, perhaps the most picturesque being an account many years ago by Sir Hans Sloan of a wonderful Eastern plant, the Scythian Lamb, that devastated the ground for some distance around it.[1] It is a commonplace among practical men that ground becomes "sick" if a crop is grown too frequently. Careful examination, however, always shows complicating factors at work. Many years ago Daubeny [9] investigated certain cases at Oxford from the standpoint of de Candolle's excretion hypothesis but failed to find any evidence of the existence of toxins, and his work still remains among the best we have on the subject. Even the strikingly harmful effects of grass land on fruit trees demonstrated by Pickering's well-known experiments have not been shown to be the result of any toxic excretion.

On the Broadbalk field at Rothamsted wheat has been grown continuously since 1843, but there is no sign that the unmanured plot is suffering from a toxin; the young plant, which ought to show symptoms of poisoning if a toxin were present, is always healthy and develops normally. But there is the greatest possible difficulty in keeping down weeds because the opportunities of "cleaning" land under continuous wheat culture are not great, and no practical man could possibly bear the expense such a system entails. Again, continuous turnip growing leads to difficulties because the particular cultivations involved soon cause the formation of a hard layer of soil—technically a "ploughsole"—some 5 inches below the surface. Continuous mangold growing on Barnfield has brought about a diminution of the soil organic matter so that a tilth can only be got with difficulty and the soil cakes

[1] See Evelyn, *Terra*. "This vegetable is called the Tartarian lamb from its resemblance in shape to that animal. It has something like four feet, and its body is covered with a kind of down. Travellers report that it will suffer no vegetable to grow within a certain distance of its seat."

FIG. 1.—Wheat plants from Rothamsted plots

1. Broadbalk plot 9, complete artificial manure, including 46 lb. of nitrogen as nitrate of soda.
2. Unmanured since 1839.
3. Hoos field, wheat grown every other year only, no manure.
4. Agdell field, wheat grown in rotation every fourth year only, no manure.

very badly after rain. If a storm happens to come any time after the young plant is through, and before it has grown very much, there is great danger that it may be beaten down into the sticky soil and not pick up again; an excellent illustration is afforded during the current season. These cultivation difficulties are very serious from the practical point of view and effectually prevent continuous cropping with any one plant except where a liberal amount of organic matter is present in the soil, when tillage is a much simpler matter. A rotation is therefore always followed in practice—the benefit to the crop is seen in the following table of wheat yields at Rothamsted:

TOTAL PRODUCE LB. PER ACRE AND TOTAL GRAIN LB. PER ACRE

	Rotation. (Agdell Field.) Unmanured since 1848.		**Alternate.** (Hoos Field.) Unmanured since 1851.		**Continuous.** (Broadbalk Field.) Unmanured since 1839.		Plot 6. Complete artificial manure, including 43 lb. N per acre per annum.	
	Total produce.	Total grain.	Total produce.	Total grain.	Total produce.	Total grain.	Total produce.	Total grain.
	lb. per acre.	lb. per acre.	lb. per acre.	lb. per acre.	lb. per acre.	lb. per acre.	lb. per acre.	lb. per acre.
1907 .	3459	1066	3094	849	1715	618	5073	1554
1903 .	3219	1255	2111	915	1078	485	3175	1094
1899 .	4785	1704	2620	1004	1825	769	3182	1274
1895 .	3066	1436	2129	978	1384	664	2916	1390
1891 .	4868	1927	3645	1404	2142	828	4466	1568
1887 .	4689	2184	2365	1153	1801	906	3329	1477
1883 .	5140	2146	2461	1160	1878	872	4386	1781
1879 .	2162	669	1187	379	1093	330	2283	691
1875 .	4412	1579	2718	993	1575	567	3073	1065
1871 .	3004	929	1892	605	1715	615	3386	1089
Average	**3880**	**1490**	**2422**	**944**	**1621**	**665**	**3527**	**1298**

The 1911 crops are shown in Plate.

On Agdell field the wheat is grown in a rotation—fallow, wheat, roots, barley—wheat being sown only once in four years. On Hoos field the course is fallow, wheat, the wheat coming every alternate year; on Broadbalk wheat is grown every year. No manure is added to any of these three plots, and in the particular plot on Agdell field under consideration no clover is grown. Yet the differences in yield are striking; the wheat grown in rotation without manure is not only greater in yield than the unmanured wheat grown continuously but it even exceeds the crop on the Broadbalk plot that receives annually

a complete dressing of artificial manure including 43 lb. of nitrogen per acre.

It is difficult to account for these extraordinary results. Whitney argues from his hypotheses that rotation takes the place of fertilisers, and these figures at first sight lend some colour to his view. But the statistics for the turnip crop put a wholly different complexion on the matter. On the Agdell unmanured plot turnips give a miserably poor crop of less than a ton to the acre, against twelve tons or more on the manured plot; here then there is no evidence of rotation acting like a fertiliser. It is difficult to believe that the small excretion given off by so insignificant a crop as one ton of turnips per acre could after four years be sufficiently potent to keep the next crop down to the same low level. While therefore we cannot accept Whitney's hypothesis we are not yet prepared with another; the whole matter remains among the many interesting problems as yet unsolved presented by Agdell field.

Conclusion

The outstanding differences between Whitney's hypotheses and those more generally accepted may therefore be reduced to three:

(1) Whitney supposes all soils to be chemically alike in that all are made up of the same rock material; consequently the soil solution is the same in all cases. Other chemists, on the other hand, consider that the soil is more complex, containing colloidal decomposition products and a solution which not only differs in composition in different soils but also shows local variations in composition in different parts of the same soil.

(2) He further supposes that variations in concentration of the soil solution have no effect on the rate of growth of plants and that in consequence all soils are equally rich in plant food; added fertilisers owe their value to other than nutritive effects.

(3) He considers that infertility must therefore be due to other causes than lack of nutritive compounds; dismissing considerations of nutrition altogether, he supposes instead that infertility arises from the presence of toxic organic compounds, some of which at any rate may be plant excretions. We, on the other hand, attach great importance to the nutritive functions of the soil constituents and of added fertilisers; while some

of us agree that part of the infertility of "sour" soils may be due to toxic substances (and apparently the soils examined by Whitney and his colleagues were "sour" soils), we cannot accept the view that plants excrete toxic substances.

There is no doubt that the work of the Soil Bureau has suffered from leaving out of consideration all biological changes going on in the soil. The decomposition by micro-organisms of the residues of previous generations of plants gives rise beyond doubt to quantities of plant food, yet the function of this nutrient material is never considered; instead, attention is concentrated on possible toxic substances to the exclusion of useful substances. Thus the field of view is unduly restricted.

The investigations have, however, served a very useful purpose in stimulating inquiry and they have brought home the fact that the relationships between soils and plants are complex. It is no longer possible to take the old narrow view that the soil simply supplies food to the plant: the earlier papers compelled recognition of the fact that the size of the soil particles which regulate the water and air supply is more important than their chemical composition, and consequently that mechanical analysis is more useful than chemical analysis in characterising soils; the later papers direct attention to possible toxins of which we may have some in our own "sour" soils. We can find much to criticise in the details of the experiments and still more in the conclusions drawn from them; not infrequently the facts themselves are in dispute. Above all we should like to see a re-examination of the fundamental positions based on definite crucial experiments and consideration of alternative hypotheses. But, whether further work support their hypotheses or not, Whitney, Cameron, Schreiner and their colleagues have made agricultural chemists re-examine their ideas on the soil, and such a reconsideration must in the end advance the subject, however troublesome or superfluous it may at the time appear.

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REVIEWS

Mathematical and Physical Papers of Sir William Thomson, Lord Kelvin. Vol. iv. Hydrodynamics and General Dynamics. Arranged, revised, and annotated by Sir Joseph Larmor. [Pp. xvi + 563.] (Cambridge University Press, 1910. Price 18*s*.)

THE first three volumes of the Mathematical and Physical Papers of Sir William Thomson were published in the lifetime of the author, who also began the preparation of vol. iv.; the papers collected by him for this volume were published however as an appendix to the *Baltimore Lectures*. In the first three volumes the papers were printed in chronological order, but in this volume they have been grouped under subjects. Some of the papers continuing the sequence of the first three volumes had been published, as mentioned above, so that the original scheme could hardly be continued; and bringing together the papers dealing with one subject has distinct advantages. The time has probably not yet come when it is possible to say which place Lord Kelvin occupies amongst the great Natural Philosophers and Mathematicians of the world, but as one peruses the papers of this and the preceding volumes—there is another to follow—one feels how versatile and great the man was and that to him is assured a very high place amongst the immortals.

Addressing the British Association in 1871 Sir William Thomson remarked: "Maxwell has completed the dynamical explanation of the known properties of gases except their electric resistance and brittleness to electric force; but definite and complete as is Maxwell's theory, it is but a well-drawn chart in which all physical science would be represented with every property of matter shown in dynamical relation to the whole. But there could be no satisfaction to the mind in describing precisely the relation of the states of matter when the properties of the atom are assumed." Long before this famous utterance the mind of Thomson was searching after a comprehensive theory of matter, which not only, as Larmor says in his *Ether and Matter*, "can be formulated as a scheme of differential equations," but of which he could also imagine a model.

Helmholtz, in his epoch-making memoir "On the Integrals of the Hydrodynamic Equations which express (Wirbelbewegung) Vortex Motion," had as early as 1858 investigated the laws of whirlpool or vortex motion in a perfect fluid and had shown that once vortex motion has been set up it is permanent and that there is an unchanging relation between the rotation and the portion of the fluid acquiring the motion. Nine years later Tait had translated this remarkable memoir and devised the ingenious and well-known experiments with smoke-rings to illustrate the theory. On seeing these experiments Thomson saw the possibility that the vortex rings of Helmholtz may be the only true atoms, and the papers in the first section of this book are reprints of his papers on Vortex Motion, in which he developes the hypothesis "that space is continuously occupied by an incompressible frictionless liquid acted on by no force, and that material phenomena of every kind depend solely on motions created in the liquid."

In a lecture delivered in Edinburgh, a short account of which commences this volume, he remarks: "Lucretius' atom does not explain any of the properties of matter without attributing them to the atom itself. It is as easy (and as improbable—not more so) to assume whatever forces may be required in any portion of matter which possesses the (Wirbelbewegung) as in a solid indivisible piece of matter."

The extraordinary skill displayed in finding the solutions of given mathematical problems is only surpassed by the insight, clearness of thought and power to translate physical phenomena into mathematical language shown in these pages. The reader feels that here he is dwelling with one who is on the verge of wresting from Nature some of her hidden secrets; but alas! with all the wonderful insight and mathematical skill, the theory could not yield a satisfactory explanation, to use Thomson's own words "of universal gravitation or any other property of matter"; and even less could he see hope of it "explaining chemistry and electro-magnetism." The quest was a grand one and he pursued it unflinchingly, even though he had to admit, ten years after the time of writing the last of the papers on Vortex Motion given in this volume, that it was to the end—failure.

The second part of the book consists of three papers on the "Theory of the Tides," the first being the famous paper "On an alleged error in Laplace's Theory of the Tides," in which he defends Laplace against Airy and Ferrel, showing that the procedure of Laplace was correct; with rare skill in this and the following two papers he developes and improves the beautiful theory of the famous French mathematician.

The third part is closely allied with the second, consisting of eleven papers on "Waves on Water," a subject, as also that of tides, to which the author, from a practical and theoretical standpoint, the former not being dealt with in this volume, made such notable contributions. The last paper in this section is a short note on the "Physical Explanation of the Mackerel Sky." All that is essential to the formation of a mackerel sky "is that portions of air should be moving up and down; and further, that the up-and-down motion should seem as though it resulted from the slipping of one stratum of air upon another and the production thereby of waves; and the second essential is that one or other of the two portions of air should be very near the point of saturation—that it would be clear when down at its lowest point and cloudy when up at its highest." Space forbids many remarks on the papers on Waves, but in the one on "Deep-Water Ship-Waves," written ten years after the last of the papers on "Vortex Motion," he admits that the motion in the Helmholtz circular ring is unstable, and if the water in the whirlpools formed by a ship "suddenly loses viscosity and becomes a perfect fluid, the dynamics of vortex motion tells us that the rotationally moving water gets left behind by the ship, and spreads out in the more distant wake and becomes lost; without losing its kinetic energy, which becomes reduced to infinitely small velocities in an infinitely large portion of the liquid." The fourth section includes a number of papers on General Dynamics, in some of which one sees how the author was endeavouring to conceive dynamical models to illustrate the properties of matter. He deals with "Elasticity viewed possibly as a mode of motion" and concludes by suggesting that there is yet in store for the world a beautiful work bearing this title.

In the paper "On the Gyrostatic Working Model of the Magnetic Compass," it is shown how gyrostats suspended in suitable manners can be made to act as a dipping needle and magnetic compass respectively; and in an abstract from a lecture on Gyrostatic Experiments given at Belfast, he expresses the belief that

"if ever we are to solve the difficult problem of the elasticity of matter, it will be by the aid of the phenomena of rotation"; and later, in a sentence he reveals his attitude not only to the study of gyrostatics but also, one thinks, to the study of mathematics in general. He declares he is not content with the study of gyrostats because they are interesting in themselves but because he realised "that in reconciling the laws of light, the laws of magnetism, the laws of electricity and the laws of matter, they must play an important part."

The volume concludes by a chronological list of the titles of papers on "Elastic Propagation," with references to the volumes in which they have been published, interspersed by six short papers not previously reprinted. Three of the six are of electrical or of magnetic interest, and a fourth deals with the "Duties of Ether for Electricity and Magnetism."[1] The Elastic Solid theory of Ether was incapable of accounting for magnetic phenomena and Thomson now puts forward as a tentative theory the hypothesis that two portions of ether occupying infinitesimal volumes V and V′ at distance D repel mutually with a force equal to

$$\frac{(\rho-1)\,V.(\rho'-1)\,V'}{D^2}$$

ρ and ρ' being the respective densities of the two portions of ether considered. If either $(\rho-1)$ or $(\rho'-1)$ is negative, the two portions attract each other, and if either becomes zero there is no repulsion. From which it follows that ether of undisturbed natural density experiences neither attraction nor repulsion from the ether, which the author premises is condensed into a positive electron, or from the rarefied ether in the space occupied by a negative electron; whereas the ether in two positive or two negative electrons would repel each other and there would be apparent attraction between a positive and negative electron.

This, except for a short paper on "Stress and Strain," brings the volume to an end. To future investigators this and the other volumes will prove invaluable, bringing as they do together so many of the splendid contributions to Physics and Mathematics of perhaps the greatest of English men of Science of the nineteenth century—yea, perhaps the greatest since the author of the *Principia*. To the serious students of mathematics no training can be finer than that entailed in the disciplinary study of the problems presented in these papers, in which imagination enlarges and makes to live that which in the hands of the pure mathematicians is apt to be the dry bones of severe formalisn.

F. C. Lea.

Light, Visible and Invisible. Second Edition. By Silvanus P. Thompson. [Pp. xiii + 369.] (London: Macmillan & Co., 1910. Price 6*s*. net.)

The first six chapters of this book are a reprint of the first edition and consist of six Christmas lectures delivered at the Royal Institution in 1896. To these have been added a lecture on Radium given in different places in 1903–4, and a lecture on the Manufacture of Light delivered in 1908 at the York meeting of the British Association.

There is no attempt at a complete exposition of the subject and higher mathematics is intentionally absent (a few elementary proofs are given in appendices); but it is surprising how much is included and satisfactorily explained.

[1] See Preface to *Baltimore Lectures*.

The subjects of the juvenile lectures include reflection and refraction, the visible and invisible spectrum, polarisation and Röntgen light; the wave theory is used from the beginning. The lecture on radium is an interesting account of the first discoveries in connection with this element. It is, of course, very short and incomplete, written, as it was, six years ago. The last lecture, on the manufacture of light, is an inquiry into the properties of our present modes of illumination with a view to the discovery of one of higher efficiency.

The whole forms an interesting and readable book. A text-book it does not profess to be but the reader will find in its lucid summaries and simple expositions a means of clarifying confused ideas which may have been obtained from the detailed accounts and technical language of more formal treatises. The demonstrations are striking, instructive and described with sufficient detail to enable any lecturer to repeat them. The book is very well printed and illustrated, and, within its limits, fulfils its purpose admirably.

H. MOSS.

Heredity in the Light of Recent Research. By L. DONCASTER, M.A. The Cambridge Manuals of Science and Literature. (Cambridge: at the University Press, 1910. Price 1*s*.)

WE were told in our youth that oases are met with but infrequently in arid deserts and are usually small: if this be the case the work before us may well rank as an oasis in the vast desert of arid literature presented to readers in these days. Mr. Roosevelt, in his *African Game Trails*, speaking of the books he has carried with him on his travels, remarks: "I did not often take scientific books simply because as yet scientific books rarely have literary value. Of course a really good scientific book should be as interesting to read as any other good book." Of course it should and if books can be produced like the all too brief essay under notice the stigma attached by Mr. Roosevelt to scientific books will be removed at no distant date, so that the Hunter-President will be able to include some of them with *Alice in Wonderland*, *Huck Finn* and Omar Khayyam in his "Pigskin Library" on his next expedition into the wilds of Nature.

No study can be of greater importance to mankind than that of Heredity. The situation is pithily defined by Mr. Doncaster in a short reference he makes to Eugenics, "the study, that is, of the methods by which the race may be improved both physically and mentally." "The whole trend of the results obtained [he says] is that in order to produce exceptionally gifted men in both body and mind, those with high development of the characters desired should be encouraged to marry; and that to prevent the production of the weakly and feeble-minded, the only method is to prevent such from having offspring. It is admitted that at present these things hardly come within 'practical politics' but there is little doubt that the nation which first finds a way to make them practical will in a very short time be the leader of the world."

The only objection we are inclined to urge is that the book contains far too many of the special terms with which the Mendelians have thought proper to load the study of their subject—so much so that it may almost be supposed this is done with the object of making it forbidding to the general public. No policy could be more shortsighted than that which is so widely adopted at the present time by scientific workers of coining special terms to express each new nebulous conception as it arises, whether or no there be real need of such expressions.

The Tower of Babel must have been a place in which intercourse was easy in comparison with that afforded by the platform of modern science.

One request we venture to address to Mr. Doncaster—that with loving care, such as Stevenson and Lafcadio Hearn exercised on their writings, he will revise his booklet and here and there make the slight change that will give to each sentence the exact shade of meaning he would wish it to carry; further, that he will consider if he cannot assist in promoting the reform that is needed to make punctuation of some service to the reader. Unfortunately, compositors and printers'-readers have acquired certain tricks and nearly always sow commas to spoil conjunctions and in many other ways to interrupt sentences for no discoverable good reason. If Mr. Doncaster would set an example when and where conjunctions should not be used, he would do service to young writers and might be referred to by Sir Clifford Allbut in his *Hints on Essay Writing* as an example to be followed. For instance, taking the opening paragraph of the first chapter, nothing could be better than the earlier part; the final sentence is not so felicitous—the comma before the first *and* as well as the second *and* are both unnecessary and disturbing; moreover, secondly and first might well be brought into agreement. It is because the book as a whole is so charmingly written that minor blemishes such as these—which perhaps a majority of readers will disregard—are disturbing; we trust we may not be accounted altogether pedantic in preferring this request: our feeling is that unless more good examples can be set, style must disappear altogether from our scientific literature.

Alcoholic Fermentation. By ARTHUR HARDEN, PH.D., D.SC., F.R.S. Monographs on Biochemistry. (Longmans, Green & Co. 4*s.* net.)

THIS book will be of the greatest service to all serious students of the subject discussed in its pages and Dr. Harden is to be congratulated on the manner in which he has pieced together a clear and concise account of the gradual growth of knowledge of the nature of the process of alcoholic fermentation; he has executed an exceptionally difficult task with conspicuous success. The opening "Historical Introduction," written with the author's well-known literary skill and with much feeling—the feeling of one who *knows* the subject he is writing about—is a model of lucid exposition; moreover the picture presented is very fascinating and of extraordinary interest as an illustration of the slow and gradual way in which conclusions were arrived at that students, at the present day, are led to treat as commonplaces, if not as self-evident propositions—either because they are fed to them in "potted" form by their teachers or because they gather them from their text-books without in the least understanding the logical process or method underlying the inquiries of which the conclusions are the outcome.

Chapter II. contains an account of the preparation and properties of Zymase, the fermentative principle of the yeast cell separated by Buchner in 1897, the discovery of which must ultimately be regarded as one of the great epochs in biochemistry. The author's discovery that a phosphate is an absolutely essential member of the company of agents concerned in the process of fermentation is fully dealt with in the third chapter. The fourth is devoted to the co-enzyme of yeast juice—the mysterious substance which apparently co-operates with the enzyme and phosphate in determining the resolution of the sugar molecules into alcohol and carbon dioxide. The action of certain inhibiting and accelerating agents is considered in the fifth chapter. The sixth is devoted to the by-products

of alcoholic fermentation; lastly, the chemical changes involved in fermentation are discussed in the seventh chapter, the mechanism of fermentation in the eighth.

Here and there a few obscurities in language are to be met with; these, it may be hoped, will be removed in a later edition. Thus (p. 85) we read: "This enolic form is capable of giving rise to all three hexoses and the change by which the enolic form is produced and converted into an equilibrium mixture of the three corresponding hexoses is catalysed by alkalis or rather by hydroxyl ions." "Equilibrium mixture" is ugly, "catalyse" is a hideous and unnecessary verb; in what way are we to think of a change as "catalysed" or "loosened down"—and why the "or rather by hydroxyl ions"?—why should we not be satisfied with alkalis, which are real things; why not leave alone hydroxyl ions, which are mere metaphysical abstractions; and if ions are to be spoken of in such a connection, why leave out the poor sodium ion which has just as much right to consideration as the hydroxyl ion in such a case, Faraday having taught us that two opposite ions must be operative simultaneously? There seems to be no end to the absurdities that can be perpetrated in the name of ions: it is most desirable that practical people like Dr. Harden, who have command of our language, should set the example of using plain English when writing for English students. The path of the student is sufficiently strewn with obstacles without casting thorns upon it; let any one who understands the subject consider the difficulty a student would have in construing the three and a half lines referred to.

The chief feeling of the expert reader will be one of gratitude to Dr. Harden for having brought the facts together so completely and conscientiously; but it is not to be expected that in a subject of such difficulty the meaning of the results arrived at by various workers will at once find a proper interpretation. It is only too obvious on turning over the pages of the book that we are but at the beginning of the inquiry: we know that the process of fermentation can be made independent of life, we know that it is an enzymic process, yet one apparently in which a variety of agents are concerned: that is all; at present we cannot trace the process in any of its details. But this little all marks an extraordinary departure from the views that were current and are current even now that phenomena apparently so complex as are those of fermentation must have their origin in the processes of life.

We may hope that Dr. Harden will be able to shed yet further light on a subject which he has made so peculiarly his own of late years.

Lessons on Soil. By E. J. RUSSELL, D.Sc. [Pp. xv + 115.] (The Cambridge Nature Study Series. Price 1*s.* 6*d.*)

THIS is the first volume of a Nature Study Series to be issued by the Cambridge University Press and the Syndics are to be congratulated most warmly on their successful beginning. It is a book to be read and pondered over not only by children at school but by every intelligent person, young or old, who can take the least interest either in gardening or agriculture. It is written by one who knows what he is writing about and is able to express himself simply, clearly and concisely. Nature Study will become of some value and not the farce it is in too many schools, if a few such books as this can be produced and put into children's hands and those of their teachers. The illustrations are exceptionally good and numerous; that on p. 13, it may be mentioned, is of the landslip near Lyme Regis, Dorset, not of the slip in the Isle of Wight.

The Coast Scenery of North Devon. Being an account of the geological features of the coast-line extending from Porlock in Somerset to Boscastle in North Cornwall. By E. A. NEWELL ARBER, M.A., F.L.S., F.G.S. Illustrated by 70 photographs, 12 text-figures, and 2 maps. [Pp. xxiv. + 261.] (London: J. M. Dent & Sons, Ltd., Bedford Street, Strand, W.C. Price 10*s.* 6*d.* net.)

PERHAPS the chief value of this book lies in its illustrations; there are no less than seventy large plates, from photographs taken by the author, portraying the characteristic structure displayed in various sections along the coast of North Devon between Porlock and Boscastle. With few exceptions, they are most beautifully executed and should be of value in bringing home to students the features of a coast-line which is inferior in pictorial interest only to that of the neighbouring county, Dorset. In one or two cases the distance is very poorly rendered and the author has been unfortunate either in his choice of plate or more probably of weather: to take such pictures properly it is necessary to go not once but several times and to study and secure the conditions under which alone justice can be done to each subject. Although an expert, Mr. Arber does not seem to have quite the soul of a real photographer—in no single picture is the sky otherwise than blank; surely there are conditions under which beautiful cloud effects are to be secured in North Devon. Is the author perhaps unaware of the value of a colour screen and of backed colour-sensitive plates for such work?

A striking feature in the book are the many photographs of waterfalls on the coast, which will be of interest to not a few and probably make the book of special value to experts; the author indeed claims, with justice, original value for his treatment of the evolution of coastal waterfalls, of sea-dissected valleys and the marine denudation of inclined and folded rocks. The volume is intended to deal solely with the nature and origin of coast scenery. Mr. Arber is evidently an enthusiastic shore-scrambler in North Devon and gives valuable hints to would-be explorers of the coast-line. His book is intended neither as a geological text-book nor as a guide-book but to be of use to visitors who are interested in geology in a greater or less degree. Such a book deserves to be welcomed: nothing is more exasperating than the aimless existence led by the average "intelligent" visitor to the sea coast and it is time that something were done to interest him in things. Mr. Arber has done service to this class in particular; but we must confess to feeling that his work is far less likely to inspire enthusiasm than a book such as Mr. Clayden's *History of Devonshire Scenery*—and we also wish that he had put a little more fire and imagination into his pages and made them a little more readable and informing, less full of dull descriptive matter. Hints might well have been gained from Charles Kingsley's *Lectures on Town Geology*. Still, the book has so much in it that is charming that such faults of omission may be regarded as venial.

A. G. P.

The Mechanism of Life. By DR. STEPHANE LEDUC. Translated by W. DEANE BUTCHER. [Pp. xv + 172.] (London: Rebman, Ltd., 129, Shaftesbury Avenue. Price 6*s.* net.)

THIS book is of interest not only on account of the subject-matter but also from a psychological point of view, as proof how easy it is to carry the argument from

analogy too far—how easy it is to be scientific within a narrow region and outside this a barbarian. The author gravely defends the doctrine of spontaneous generation on the evidence afforded by certain purely inorganic growths, formed by such processes as that of dropping a lump of fused calcium chloride into a solution of an alkaline carbonate, phosphate or silicate. Dr. Leduc talks learnedly of osmotic phenomena and seems to have considerable feeling on this subject—but apparently he altogether lacks feeling on the bio-chemical side; otherwise he could scarcely fail to have realised that external form is of small account in comparison with the internal structural complexity of organisms, a complexity which must necessarily be absent from all structures obtained by following his methods. Arguments such as the author uses are invalidated by the fact that Nature works with comparatively so few materials, with but three of the six possible glucoses, for example; and particularly by what is now known of enzymes and their functions. Those who have had the good fortune to witness the demonstrations given by Dr. Butcher on various occasions of late will be aware, however, that the growths described by Dr. Leduc are extraordinarily beautiful and interesting. Moreover, the observations are undoubtedly of real importance, as throwing light on the possible character of some of the processes of cell growth in which osmotic changes are of consequence. They are, in fact, to be taken seriously, although the main conclusions based upon them cannot for one moment be accepted.

MATHEMATICS IN ENGLISH SCHOOLS

By CHARLES GODFREY, M.A.
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Aristocratic education is a form of education designed to meet the needs of the ablest boys; it is the system on which a good proportion of present-day teachers were trained: the method adopted was that of wholesome neglect, a very good method when the subject is a clever boy and the teacher a real master. Those of us who were pupils of Mr. R. Levett at Birmingham, for example, appreciate the self-restraint with which he taught his scholarship boys and the independence fostered by his policy.

The aristocratic theory of education has determined the choice of subject-matter in teaching mathematics in schools and in this particular we are beginning to depart from the theory nowadays. In the past—the not distant past—the assumption was made tacitly that mathematics could appeal only to the few; that the average boy was essentially stupid and more or less a hopeless problem. This entirely false assumption arose from the aristocratic theory: the course was designed for the best boys, with the object of turning out mathematical scholars; in this it was bound to be successful, as the number of scholarships available was predetermined: whether or no it was equally successful in turning out mathematicians is another question. What it failed to do was to train up a generation of men capable of thinking in a mathematical way and of understanding the way in which mathematics enter into modern life.

It is not surprising that these objects were not attained. In geometry the matter and methods of instruction were purely Greek. Now the material side of Greek civilisation was based not on mathematics but on slavery. Greek civilisation depended on forced labour; our civilisation depends on the forces of nature, which need more refined methods of subjugation. When we speak of Hellas, we have in mind the cultured few; to them mathematics meant a good deal but solely as an

intellectual luxury. It may be conjectured that mathematics will never be the favourite intellectual luxury of the educated Britisher; if the subject is to appeal to him, the appeal must come from another side. And here I do not want to be suspected of maintaining that mathematics can appeal to a Briton only as a means of filling his pocket. It is difficult to turn mathematics into much money; a distinguished engineer has told us that he can buy all the mathematics he needs at a very moderate number of shillings per week. Mathematics is not a bread-and-butter subject except for those who are satisfied with this simple diet. The argument must be put on a higher plane.

Modern civilisation stands on a foundation of applied mathematics; without mathematics the earth could not support its present population. This statement will not be controverted by any one familiar with the work of modern scientists and engineers and with the part played by mathematics in their achievements. The average man takes no direct part in these developments but it is not fitting that he should live as a mere parasite on the organisation that keeps him alive. It is the work of the few to develop steam, electricity and machinery; to render navigation secure by predicting years ahead the motions of the heavenly bodies; to join the continents together with steamship lines and electric cables; to cover new corn-bearing lands with railways; to tunnel the Alps and the Andes; to prepare for the day when our coal will fail by extracting electric power from water-falls and tides. These are some of the contributions of science to our means of existence; and mathematics is one of the chief tools that science uses. The modern man should have at least some conception of the means by which these results, so vital to him, are obtained. Geography will teach him what is being done and how, directly or indirectly, his life is affected thereby. Mathematics and science will teach him how these things are done.

Here I am urging the "outlook" value of mathematics, rather than the utilitarian. I am assuming that the majority of boys will not make any direct use of mathematics in after-life, that they will not even be able to follow in detail the mathematical methods of engineering and applied science. But they can be so taught that a vista is opened through which may be seen the tremendous potentialities of the study of which

they are mastering the elements. They should be brought to the stage from which a broad undetailed view may be obtained over the country of applied mathematics: and they may be shown the beginnings of a few of the roads that lead through this country. A public must be created able to realise what science and mathematics are doing for the world and to form some general conception of the means used.

In England we have a ruling class whose interests are sporting, athletic and literary. We might easily have a worse set of masters: but they do not know or if they know do not realise that the Western civilisation on which they are parasitic is based on applied mathematics. This defect will lead to difficulties; it is curable and the place for curing it is school. The study of science in public schools will do much to put this right; but science has not the privileged position that mathematics enjoys: it has not the same opportunities. Mathematics was a well-established subject of instruction in public schools before science was heard of. During many generations English boys have learnt mathematics but the subject has not been taught in such a way as to cultivate a mathematical outlook on the world. It has not justified its privileged position. Partly this is due to the aristocratic theory of education. We have to break this down and think of the average boy; the clever boy will take care of himself and there is no fear of his interests being forgotten so long as scholarships are so powerful a lever in education. The aristocratic theory has been the bane of education in all subjects; the specialist teacher wants to make a man in his own image: the non-specialist, who might be expected to sympathise with average wits, lacks either the courage or the originality to strike out a line of his own. It is to the specialists that we must look for improvement. They must be persuaded that it is not necessarily deplorable for their pupils to remain ignorant of dodges and bits of knowledge that they themselves cherish from long familiarity.

But apart from the aristocratic theory the main reason why the average boy leaves school with no mathematical outlook is that the cultivation of such an outlook has not been accepted as a main object of mathematical instruction. Why not? There is no glory to be won from slaying the slain and I am tempted to remain silent on the subject of examinations. Examination

grinding has been attacked so often and defended so seldom that in theory it should be quite slain; but I have an impression that most of the testimonials I have read recently contain reference to Mr. A.'s success in preparing his pupils for examination; I doubt whether things have changed perceptibly for the better. Most teachers still regard a good examination average as the most real test of success; the "mathematical outlook" developed under these circumstances is likely to be a sharp outlook for a probable question. This will continue to be the dominant feature in our system until the time comes when a teacher is trusted to examine his own class. In the meantime there is a minority who sow for a harvest to be reaped elsewhere than in the examination room. What are their ideals?

A section—perhaps the larger section—are victims of a psychological theory; the theory being that in the class-room you can develop certain powers or faculties that admit of being carried over and transferred to the activities of real life. Some hold that the memory can be so trained and developed, though it is fair to add that memory training is commonly attempted through the medium of literary rather than of mathematical studies; respectable teachers of mathematics fight shy of much memory work. However this may be, it is probably impossible to increase the general retentiveness of the memory by any kind of exercise; the memory is not analogous to a muscle. To come nearer to mathematical practice, it is almost universally assumed that the study of the Euclidean and other derived systems of formal geometry cultivate the so-called "logical faculty." Such studies do no doubt attain their object when the subject-matter is geometry: by studying formal geometry I became more wary in approaching a geometrical argument and more skilful in detecting fallacies in such an argument. But did I become more logical in the reasonings of actual life? I should like to answer in the affirmative but some evidence ought to be forthcoming before this answer is given. I should like to see evidence that mathematicians actually are more logical beings than their fellow men (apart altogether from the question of whether it is well to be logical). I should also like to see a psychological discussion of this logic business; it might be found that the word "logical" is used in more senses than one. If a trained

geometrician be expert in putting his finger on the fallacies in a mathematical argument, is not this mainly because he knows the kind of thing he has to look out for? The fallacy may arise from a bad figure, from the neglect of an alternative root to an equation, from assuming that an infinite series must diverge and so forth; all these are old friends. I do not know that a mathematician would be quicker than another man to unveil the fallacies of a free trade or protectionist argument, unless the argument were of a mathematical nature; the weaknesses of political arguments usually arise from erroneous historical premises. If a training in geometry were of palpable value in fortifying the logical faculty (assuming that such a thing exists), it ought to be easy to find definite instances in which it might be said, "So-and-so would not have made this mistake if he had learnt Euclid." But is it easy?

We are very much in the dark about these questions. I should be unwilling to go to the stake in defence of the statement that Euclid has made me logical. Even if mathematicians do exhibit certain well-marked characteristics in their habits of thought and behaviour, we have at least two hypotheses before us: the characteristics *may* be due to their course of study; or on the other hand these *may* be innate characteristics which led their possessors to choose the study of mathematics. Even if we adopt the former hypothesis, we have to remember that the fully fledged mathematician has made a very intense and concentrated study of his subject. He has probably studied nothing else during at least three years at the university and very likely during two years before; and he may have been engaged in mathematical work and teaching an indefinite number of years before he comes under our observation. An ordeal of this duration does undoubtedly tend to colour the whole mental and moral character. But what we are concerned to ask is rather this, Does a comparatively superficial study of mathematics (or any other subject), such as may be made by a schoolboy not specially gifted in this direction—does such a study as this affect seriously his general mental qualities? Such a study may do much for a boy. It may give him definite knowledge, it may give him interests, it may give him ideas and modes of thought about form and quantity that will shape his way of looking at all sorts of subjects; it may give him outlook and intelligent appreciation of things going on round

about him. So much I think we may make fairly sure of if we go to work in the right way and I wish to make some suggestions later on as to how these ends may be attained. But when it is said that mathematics develop the memory, the logical and reasoning faculty, the power of generalisation—develops all these powers as applied not only to mathematics but also to general activities—well, I hope that it may all be true but I have not met with the proof. And unless these developed powers can be carried over from mathematics into general activities, the effect of the teaching is of no significance in the education of the average boy ; the average boy is not being trained for a mathematical career.

Boys certainly are apt to leave behind them in school any good habits that they may have formed there. They go even further and leave their habits in the particular class-room where these were engendered. Cases of this kind are typical : A boy who is practising decimals in mathematics is found unable to divide by a thousand in the laboratory ; he may be studying cylinders in mathematics but breaks down over the sectional area of a cylinder in the workshops ; a senior class is bowled out by their classical master over a question of sesterces. These cases are too familiar to be a matter of surprise to any one who supervises the work of more than one department ; but to the specialist they are a continual offence. The only remedy is to have a good splice between different departments, so that what is learnt in one class-room may be applied in another. A mathematical question set in a physics class-room takes a boy unawares ; it comes in a strange form—it is an unusual stimulus and does not automatically provoke the expected reaction. We want to make a boy's knowledge and acquired habits responsive to all sorts of stimuli ; hence the need for that system so dear to educationists and such a bugbear to practical teachers—correlation.

The point of this illustration is, that if it be so difficult to "carry over" mental habits from one department to another within a school, how much more difficult is it to "carry over" from school into after-life. Perhaps this object would be better attained (if it be attainable) by providing for it more deliberately. For instance, the study of geometry ought to teach that it is necessary to verify hypotheses. The framing of hypotheses depends on a cultivated power of guessing, a

kind of "flair." This guessing is a necessary part of mathematical thought but the guess has to be verified in some way or other, in geometry generally by deductive reasoning. In solving a geometry rider I may guess or suspect or have an intuition that a certain pair of angles are equal; I may be unable to solve the rider till I "spot" this and the guess may be half the battle. The other half consists in verifying the hypothesis. Now it is of no avail that a boy should be habituated to verify his hypotheses in geometry unless he be able to "carry over" the habit into real life; the commonest cause of vulgar errors is the failure to verify hypotheses. But it is very doubtful if the habit is often carried over, and I think that good work would be done if boys were taught explicitly that this habit is formed for them in school in order that they may apply it out of school; probably very few would realise this unaided.

Whether the faculty-training view of mathematics be right or wrong, it has held the field during several generations and there has been every chance of ascertaining what it can do. What are the results? A referendum would certainly lead us to say a failure. Whether or no we have done all that is possible, we have certainly failed in one thing: broadly speaking, we have failed to make mathematical thought enter as a main element into the life of the educated classes. To redeem this failure is work for the present generation of teachers and I suggest that we shall succeed if we think less of the faculty-training and more of the "outlook" aspect of mathematical teaching.

What is there in the present syllabus to give boys a mathematical outlook? For the average boy the syllabus comprises arithmetic, algebra and geometry and nothing else. My thesis is that the treatment of these subjects should be so remodelled as to leave time for a further range of subjects and a wider field of ideas. Considering the time given to the subject—largely in excess of that given in France or Germany—the amount of mathematics covered by English schoolboys is insignificant. To the faculty-trainer this is a matter of indifference, as the training can be given as well through a thin syllabus as through any other. From the outlook point of view it is not a matter of indifference.

More and more the affairs of life are being made amenable to mathematical treatment and, as it has turned out,

the development has been on lines divergent from the lines of schoolwork. The form of mathematics that is being introduced nowadays into so many departments of thought is the infinitesimal calculus. For general purposes, though the technicalities and machinery of the calculus are not needed, the language, notation and ideas of the calculus are of all-pervading utility. Leaving aside the definitely mathematical sciences of physics and engineering, we find the calculus entering into studies such as chemistry, biology, economics and statistics. Lectures on these subjects are apt to be unintelligible to an audience brought up on school mathematics, for want of a nodding acquaintance with the calculus; at some universities it is found desirable to arrange special mathematical lectures in order that students may be able to follow the instruction in other subjects, a kind of deathbed repentance for those who have wasted their mathematical time at school through no fault of their own. And let it be noted that this simple form of calculus does not grow out of the summit of school mathematics but branches off low down the stem and it is independent of formal geometry; a vigorous pruning of school algebra and arithmetic would in no wise prejudice the growth we want to encourage.

The infinitesimal calculus has now been before the world during two and a half centuries. It is the fundamental form in which mathematics are applied to the affairs of modern life. We must recognise as a law of development in educational affairs that matter which in one century occupies the attention of the foremost philosophers finds its way in a subsequent century into the elementary curriculum. If the infinitesimal calculus was the high-water mark for the seventeenth century, so was the geometry of Euclid to the third century B.C., the Arabic notation (in Europe) for the twelfth century A.D., the method of long division for the fifteenth and so forth. The quality of the human brain does not alter, presumably, from one century to another; how then has it been possible to make the schoolboy of one generation assimilate matter that has puzzled the best brains of earlier times?[1] The answer is simply this: Teachers come to know more, simpler methods of presentation are discovered and it is seen more clearly how

[1] Wallis, Newton's forerunner, was very doubtful whether he might write $\sqrt{8}$ as $2\sqrt{2}$.

the curriculum can be disencumbered of the obsolete and the unessential. No one who has taken the trouble to acquaint himself with the present world-movement in education will doubt that one of the tasks of the twentieth century is to find a way of importing the notions of the infinitesimal calculus into the ordinary school curriculum.

The world has moved in and left school mathematics in a backwater. Geometry stands a venerable monument of antiquity on which I will lay no sacrilegious hands. But mathematics is applied to modern life in an analytical or algebraical not a geometrical form. Newton revolutionised scientific thought with a geometrical treatise but he had arrived at his results by analytical means, inventing the calculus for the purpose; his successors have not thought it necessary to clothe their work in a garb which respectability in those days demanded. Formal and demonstrative geometry is not going to help us very much on the side of outlook; it must be taught as mental training, for we can hardly break with the training theory entirely, though our hold on it may be weakening.

Now consider school arithmetic. What does it contain? It teaches in the first place ordinary ciphering, a necessary art. It teaches all sorts of operations with English weights and measures and coinage—mostly superfluous. It betrays its commercial origin by treating of a number of commercial rules which may have been practised in the City in Cocker's day. But it is mainly a complete collection of methods for solving all problems that have been set by all examiners since the invention of printing, a snowball that still grows, a burden to boyhood, a nightmare to mathematicians. "'If this were only cleared away,' they said, 'it would be grand.'" If it were cleared away we might discern the true simplicity of arithmetic. The invention of logarithms has left much of arithmetic on the scrap-heap, a shrine where it is still worshipped. There is no outlook to be got from arithmetic; the main thing is to have done with it rapidly and ever after to use it as a tool.

Algebra is perhaps the mathematical subject which gives the smallest return for the time spent on it and I shall indicate the cause of this.

It is, I am afraid, a fact that in this country the main preoccupation of teachers of algebra is to impart to their pupils a high degree of mechanical manipulative dexterity in

handling algebraic expressions. Now this statement does not imply that teachers try to make boys manipulate algebraic expressions without understanding what they are doing. I am not speaking of ordinary bad teaching; but I am saying that a great number of very competent teachers are inspired by a wrong ideal. They want boys to understand in order that they may manipulate correctly, whereas their ideal should be just the reverse. The ultimate aim should be not manipulation but understanding and outlook.

English education is dominated by examinations. Examiners cannot test outlook and they can test understanding only by testing manipulation. Teachers have to supply what examiners demand. It is not surprising then that many teachers have mistaken the means for the end. A vast amount of time is spent on purely mechanical work: highest common factor, fractions and factors beyond the types needed for practical purposes, needlessly heavy equations, together with all sorts of artifices and elegancies which are to the average boy as pearls to swine.

Now if a boy be certainly destined for a career in which he will be bound actually to make use of mathematical manipulation, a case might possibly be made out for drilling him at school to a high degree of dexterity in the technique of algebra, just as a student who aspires to be a professional pianist must devote an astonishing number of hours to the technique of playing the piano. But for the moment we are considering the case of the general student of mathematics, the non-specialist. We may assume that the average man, not connected with any mathematical or scientific profession, finds practically no occasion in the affairs of life to enter into the details of an algebraic calculation; it is even more certain that if such an opportunity present itself to him exceptionally the opening is declined, in spite of (perhaps because of) the heavy drill that has darkened his schooldays. On the other hand, those of us who believe in a mathematical training are profoundly convinced that such general mathematical ideas and modes of thought as may be wrought into the mind by a suitable course of instruction are an asset of permanent cultural value. It is these ideas and modes of thought that we regard as a necessary element of a liberal education; manipulative dexterity on the other hand we look upon as a purely *specialised* technical accomplishment.

It is a specialised technical accomplishment, just as much as dexterity in glass-blowing or Latin verses or machine-drawing or playing the piano or shorthand—all of them very excellent things in the right place but by no means essential to a liberal education. If there were no other way of using mathematical time than in giving this technical skill I should say at once: Cut off some time and give it to English or science or something else of general cultural importance. But naturally I do not take this view; on the other hand I am persuaded that a drastic abatement of this juggling with algebraic symbols would free enough time to put every ordinary boy in possession of the fundamentals of trigonometry, mechanics and the calculus. Mathematics would gradually become, for the nation at large, a thing of real significance; we should no longer have headmasters writing to the *Times* about "the transient but blighting shadow of $x+y$ cast over their schooldays."

The question of lightening the algebra syllabus is ripe for solution. The Mathematical Association Committee has taken this particular problem into consideration and at the beginning of the present year issued a report containing definite suggestions for immediate action in this sense. The report is entitled *The Teaching of Elementary Algebra and Numerical Trigonometry* (G. Bell & Sons, Ltd., Portugal Place, Kingsway, 3*d*.).

In it a hope is expressed "that it will be possible to influence the demands of examining bodies in such a way that the teachers will have freedom to put to better use much of the time at present spent on the elaboration of algebra in elementary classes. Many teachers wish for opportunity to develop with their pupils mathematical ideas that they feel to be of greater educative value—ideas drawn from mechanics, mensuration, solid geometry, infinitesimal calculus and more especially numerical trigonometry. Custom, represented by public examinations, has at present the effect of withholding that opportunity." Throughout the report the dominant demand is "freedom for the teacher to use to better advantage the time at his disposal." There is no attempt to constrain conservative teachers to abandon their well-understood ways; examiners are merely requested to set "questions involving numerical trigonometry and other subjects that it is desirable to introduce . . . as alternatives to questions now set on parts

of the work here considered to be non-essential." Again "we do not wish to fetter any teacher in his endeavour to provide what he considers the best general education for his pupils; we wish rather to restrict the demands of examiners to things of real moment, in order to give greater freedom to educators who are anxious to progress."

It is interesting then to see what are the principles on which the report determines the portions of algebra that are "of real moment." The first principle appears to be as follows: "That within the range of work selected the teaching should be thorough, so that at each stage a boy may acquire the facility necessary to enable him to pass on to the next stage without being hindered by lack of skill in the preceding manipulations."

This should dispose of the fear that it is intended to frame an algebra course consisting entirely of ideas, an airy nothing with no substantial element for a boy to get his teeth into; a diet like Falstaff's "twopennyworth of bread to such a monstrous deal of sack." This is not the intention at all—"within the range of work selected the teaching is to be thorough." But the work selected should form a series of stepping-stones leading somewhere, not a parade-ground for practising the goose-step.

The second limiting principle is as follows: "That a boy should not be required to possess more manipulative skill (in algebra) than will enable him to deal with such parts of the subject as for the reasons either detailed above or otherwise generally admitted ought to form part of a liberal education; in other words, manipulative processes should be developed, in the elementary course, just so far as they are really subsidiary to the aims of that course and no further—not as mere curious exercises."

A description in detail of the results deduced from this limitation would probably be out of place in the present article; it is enough to indicate that the results are considerable. If algebra teaching were confined to the essentials recommended by the committee of the Mathematical Association a very important change would come over the conditions under which the average schoolboy works. At present his algebra drags on till he reaches the only goal that is set before him—Littlego or Responsions. At the age of twelve he has learnt what an equation is, what it is for and how to solve it, if it be easy. At

the age of eighteen he has looked into the matter further and found it not to repay attention: he has decided that x-chasing is not his vocation. Probably he is right and his teachers are wrong; yet whether they are right or wrong they have to satisfy the examiner, whose demands are stale but persistent.

The adoption of some such policy as that put forward by the committee might bring about something like a renaissance in English schools. This has actually been effected at Winchester, where reform on the lines indicated has been gradually brought about; the conventional schoolboy attitude toward mathematics has been changed entirely, as is patent to such impartial spectators as classical house-masters. The most serious opposition to a general movement this way will come from teachers; examiners generally cede to a fairly universal demand. There are plenty of keen, efficient teachers who cannot persuade themselves that their dear old haunts are blind alleys; year after year they go on trying to make silk purses of sows' ears, trying to turn their sensible, clumsy, ordinary pupils into skilled analysts. They are wasting their energies just as sadly as their colleague who teaches Greek to a middle form. Perhaps they would have more sympathy and insight if they had once been average boys themselves.

If time can be saved from algebra, there is no difficulty in using it. Why should a three-dimensional boy be tied down to a two-dimensional geometry? Given the time, there is much that could be done to strengthen his sense of space-intuition. There is a movement in France to amalgamate definitely the teaching of plane and solid geometry from the outset but the particular shape taken by this movement might not suit English needs; it involves a simultaneous *formal* treatment of the two branches and I doubt whether we ought to be formal in teaching "solid." We used to try this *viâ* Euclid's eleventh book but the result was poor. Still the idea of simultaneous treatment is sound, if difficult to work out. It might take the form of an informal reference to three dimensions, accompanying the formal two-dimensional course. In this way, for instance, might be introduced those simple considerations about parallel and perpendicular planes and lines that are so fundamental and yet so little understood by the general public and the matter of so much confusion of language in every-day life. There might be an amplification

of this after the plane course, with a discussion of particular solids, the use of the globes and a little descriptive geometry. The difficulty of the mensuration of solids is vanquished by the magic touch of integration.

Now that geometry has become numerical and mathematical tables no longer a luxury, trigonometry cannot be kept out of the non-specialist course. The step is easy from measuring angles on paper to measuring angles with a simple instrument of the theodolite species; there are some who would say that the step should be reversed. But real people do not go about measuring elevations without a purpose; why then should schoolboys? If they take an elevation, let them get something out of it—the height of some object in which they are interested. To do this by drawing teaches, first, what a good weapon drawing is; secondly, the nature of similar figures; the word "similar" should be heard of here or hereabouts. Now drawing is a method which no one need be too proud to use at any age but after the earlier stages of secondary school life its use should be merely incidental; when a boy has mathematical tables and has taken elevations, it is very arbitrary to withhold from him the proper way of reckoning the height by the tangent of the angle. Further than this, it is needed by the physics teacher with his inclined planes and tangent galvanometers. Trigonometry therefore cannot be excluded from the non-specialist course. I am not one of those who regret that boys find it an easy subject; there is no fear that mathematics will ever be too easy. And with trigonometry we begin to get the "outlook" which is the text of the present article. For a boy can now be brought within sight of a long row of applications to various matters of general interest and importance. This arises from the fact that the mathematical tables we teach him to use represent a vast source of energy, the stored-up brainwork of former generations, a source of energy too that does not waste as it is used. In learning trigonometry he is learning how to tap this source.

Trigonometry is liable to just the same educational misuse as algebra. The trigonometry taught in all schools twenty years ago and taught no doubt in many to-day, consisted mainly of algebraic manipulation. Teachers had altogether lost sight of the utilitarian motive to which we must appeal if we want to have the natural boy with us rather than against us.

If it is to stand in the non-specialist course, trigonometry must avoid all developments except such as have direct application to concrete and geometrical situations; to step beyond this line is to step into the specialist course and there is no time to spare for straying. The amount of trigonometry indicated may be covered in three lessons a week during two terms.

We now come to mechanics, which is often taught by the physics master. A course of physics is an essential part of a liberal education; it is almost safe to say that this fact is recognised practically in public schools and probably still more in other secondary schools. Mechanics always enter into a physics course unless it have been delegated to the mathematician; the question is not whether or no mechanics shall be taught but rather who shall teach it.

A joint committee of the Mathematical Association and the Association of Public Schools Science Masters recommended in 1909 that "statics be begun in the lower part of the 'upper school' as a part of the regular mathematical teaching, *i.e.* it should be taught by the mathematical master during mathematical hours." It may be assumed that physics masters would like to surrender mechanics to the mathematician, provided they could be assured on one point—how will the course be begun? Unless the various laws of statics are induced from experiment, the course is not likely to be of much service to the average boy. The difficulty is that many mathematical masters are unfamiliar with experimental methods; but a keen man can easily pick up enough to teach statics and he has now the opportunity of attending the course specially designed for mathematical masters held in the Cavendish Laboratory at Cambridge during the summer holidays. If there is no mathematical master available of the experimental turn of mind, mechanics might be left to the physics master. But this would be the case in few schools and there should be an increasing supply of men capable of instructing in both mathematics and physics. I am all in favour of amalgamating the mathematics and physics staff. The physics or engineering man, if he be a competent mathematician, will often be a better mathematical teacher than the pure mathematician; the latter will generally be disinclined to regard his own subject as a tool: he may treat it as self-contained, insular, having no foreign policy.

The more mathematical teaching looks outwards the better will it be for schools; the place for self-centred mathematics is the university.

The main reason for turning over to the mathematician the elementary instruction in mechanics is this, that in any case mechanics is taught by the mathematical staff to the top classes in connection with scholarship work. In the past there have been two separate and unrelated systems of mechanics-teaching within each school: the elements taught inductively by the physicist to the middle classes and the whole subject taught again, deductively, by the mathematician to the top classes. There might have been something to be said for this double system if the two courses were deliberately designed to fit in with one another but the number of schools in which this occurred must have been small. I imagine that pretty often the mathematician did not know that any other system of mechanics-teaching existed in the school: there is frequently in schools a lack of touch between departments which would do credit to a Government office. It is the business of the headmaster to see that such things do not occur; but not many classical headmasters are competent to exercise general supervision over the mathematics and science of their schools and the ignorance of headmasters is itself a result and an evidence of the failure of mathematical teaching in the past.

When mathematicians do take over the whole teaching of mechanics they will have to struggle consciously against the temptation to turn it into a set of mathematical problems. Mechanics will be valuable to the average boy in so far as it creates in him a vivid perception of laws of physical phenomena; the niceties of analytical elegance tend to distract his attention from the fundamental principles and even to cultivate wrong impressions through such abstract conceptions as the perfectly rough insect, the small elephant whose weight may be neglected and so forth. It is true that this fauna is almost extinct but we still hear too much of the frictionless machine and too little of the efficiency of actual machines; the influence of the Cambridge school of applied mathematics in its decadence is still dominant.

The case for statics in the non-specialist course is quite clear; when we come to speak of dynamics there is much difference of opinion. In these days of electric motors, auto-

mobiles and aeroplanes it would seem that every one should be allowed to gain clear notions about energy and force; but my own experience (not more recent than six years ago) is that it is very difficult to give clear notions. Perhaps there is something in the contention that boys are becoming increasingly familiar with machines and power in various forms, that all this is a very modern development and that what was difficult ten years ago may be found much easier to-day. I should not like to deny the force of this but still I should hesitate to count dynamics in the non-specialist mathematics course. General notions about work and energy should enter into statics, *viâ* the efficiency of machines; but precise quantitative knowledge concerning mass, force, momentum and kinetic energy has been attained slowly in the growth of civilisation and appears to be essentially difficult.

If we restrict dynamics to be study of motion—velocity and acceleration—we are easing the burden very appreciably. Ideas of velocity and acceleration are within the range of geometry. It is an obsolete dictum that motion—that is space, plus time—is excluded from the scope of geometry. One of the chief tendencies of geometry teaching is to use every opportunity of presenting continuous *change* of configuration. If we may for the occasion use the words statical and dynamical in the popular sense of *at rest* and *in motion*, we may say that the tendency is to make mathematics dynamical instead of statical. Mathematics was statical when it dealt entirely with things *in statu quo quiescendi*. Everything was fixed and immovable: determination of roots of equations, study of fixed geometrical figures. But now we regard algebra as concerned less with the determination of values than with the study of relations between variables. The idea of function is hovering over school mathematics; the graphing of functions has already found admittance and this is but the first glimpse of a profound change which is coming on. We are beginning to realise the meaning of the *πάντα ῥεῖ* of our schooldays; the Greek philosopher's discovery was brought within the range of mathematics by Newton's fluxious and now after twenty-four centuries it has filtered down to the schools.

When once it is granted that geometry is to tell us not only how figures stand but also how they move and change, it is a short step to the space-time diagram and the idea of velocity

as represented by the gradient in this diagram. This brings us face to face with the infinitesimal calculus.

Mr. J. W. Mercer, of the Royal Naval College, Dartmouth, has published recently an excellent *Calculus for Beginners*, the most significant feature of which is that (in development of a suggestion originally due to Prof. Perry) all the main applications of differentiation and integration are exemplified without using any function more abstruse than x^n. The first two hundred and fifty-nine pages are given to x^n and in the course of these pages the reader learns how the calculus bears upon velocities and accelerations, maxima and minima, relative errors, definite and indefinite integrals, areas, Simpson's rule, volumes, centres of gravity, moments of inertia, work done in stretching strings and by expanding gases, mean values, etc.

In all this work the manipulation is slight but the effect on mental enlightenment is immense. The importance of these illustrations is that they prove that a store of applications is thrown open by the very simplest tools that the calculus provides. The ideas, then, of the calculus and a feeling of the extraordinary power of this new instrument are accessible to a student with a modest degree of manipulative skill in algebra. It is shown that it is not necessary to tread for years the weary paths of highest common factor, fractions and the like before becoming worthy to enter this rich country.

Now it is not proposed that the calculus, in so far as it belongs to the non-specialist course, should cover the first two hundred and fifty-nine pages of Mercer's *Calculus* or anything like this amount of work. The non-specialist cannot integrate $1/x$, for this involves e. Many of the applications enumerated above will be beyond his range. For all this, there is much that he can do. He will be like the prospector picking up the first nuggets on a new goldfield. Having been trained to think functionally, he will have little difficulty in grasping the idea that the *gradient* at any point of a curve (say $y = x^2$) is a function of x. On a copy of the graph, lithographed on paper ruled in inches and tenths, he may draw tangents with a ruler, measure off the gradients by means of transparent squared paper and plot these gradients as ordinates; this will show him that the gradient is a function of x and probably he can now see what function. After a suitable amount of preparation in this style, he may proceed to determine the gradient

analytically. He is now in possession of the root-conception of the calculus and the next step will be to space-time diagrams and velocities. Next may come the differentiation of x^3, c, $1/x$ and further applications; maxima and minima will present no insurmountable difficulties. Finally the integration of such simple powers of x and the easy applications that stand at the threshold. This will be the coping-stone of the non-specialist class.

That this range of work is within the powers of the average boy at an English secondary school is proved, not only by the testimony of English schools that have made the experiment but also by the universal experience of the classical sides of French lycées. The time given to mathematics by these classical boys is shown in this table:

MATHEMATICAL HOURS PER WEEK IN FRENCH LYCÉES—CLASSICAL SPECIALISTS

Ages .	8	9	10	11	12	13	14	15	16	17	18
Hours .	3	3	4	4	2	2	2	3	2	4	2

The limit of the work undertaken in the highest class is shown by the following extract from the official programme:

"Notions de la tangente et la dérivée. Exemples de tangentes obtenues géométriquement comme limites d'une sécante (cercle, parabole). Coëfficient angulaire de la tangente; applications à quelques cas simples:

$$y = x^2,\ y = x^3,\ y = \frac{1}{x}.$$

"Notions sur l'usage de la dérivée pour reconnaître le sens de la variation d'une fonction.

"Évaluation approximative de l'aire d'une courbe tracée sur du papier quadrillé en comptant les carrés contenus à l'intérieur de la courbe; limite de l'erreur fournie par le nombre des carrés que traverse la courbe; cette erreur doit être rendue très petite en employant un quadrillage très fin.

"Aire du triangle obtenue comme la limite commune de deux sommes de rectangles dont l'une est inférieure, l'autre supérieure à l'aire cherchée. Aire de la parabole. Problème inverse de la recherche d'une dérivée. Aire d'un triangle, ou d'un parabole, obtenue par la recherche d'une fonction dont la dérivée par rapport à X est $\propto x$ ou $\propto x^2$.

"Application de la méthode infinitésimale à l'évaluation des volumes ou des surfaces des corps considérés en géométrie élémentaire."

The students in question have studied the elements of trigonometry in an earlier year. On the other hand, mechanics is not taught in mathematical time. But the marked contrast between the amount of time given to mathematics in France and England leaves us with no doubt that English boys can reach the above limit, even if mechanics be included with mathematics; probably English boys give twice as much time to mathematics as do their French contemporaries. And we need not rely upon probabilities; given the required reduction of arithmetic and algebra such a course of trigonometry, mechanics and calculus as I have outlined has been shown to be within the range of English boys by the experience of more than one school in this country; moreover the objects of the experiment can even pass Responsions.

The boy of no marked ability usually carries on his mathematics, in England, to the end of his school life but the classical specialist not infrequently is released from mathematics during the last two years. Partly this is due to the pressure of competition for specialist scholarships in classics. But it is due also and fundamentally to the belief of both boys and headmasters that mathematical studies during these two years would not be a valuable element of education. And if a non-specialist mathematical curriculum is to be confined to the cramped limits now customary here, the belief is well founded. A recognition that a wider field of ideas is accessible to the general and to the classical student would eventually raise mathematics to its true position in the appreciation of those who control our great schools and might finally have a far-reaching effect on national thought and efficiency.

TRADITION IN EDUCATION

A PLEA FOR A MODIFIED CURRICULUM AND FOR THE GENERAL RECOGNITION OF MANUAL INSTRUCTION IN SCHOOLS

By T. S. USHERWOOD, B.Sc.

"Whoever has attempted to bring about any sort of reform . . . knows well that his chief difficulties lie, not in the ignorance of those he would influence—for facts, however laboriously gathered, are readily imparted—but rather in the mental states engendered by an education which does little to create a habit of open-mindedness to new ideas."—ARCHDALL REID, *Heredity*.

RECENTLY it was the writer's good fortune to be present at the Conference on the Education and Training of Engineers held at the Institution of Civil Engineers. To judge from several of the communications considered by the section concerned with General Education, it would seem that civil engineers, alike with the general public, are not only unacquainted with modern developments in elementary and secondary education but are almost wilfully blind to the signs of the times. It was urged repeatedly that it is necessary for the engineer to have culture, that a sound general education was peculiarly desirable, yet no word was said as to the cultivation of fingers. One would have thought that the average engineer possessed the right type of mind, the cultivated imagination and the freedom from prejudice, to assess at its true worth such a subject—or rather such a discipline—as Manual Training; but to judge from the absence of all reference to this—the only logical—introduction to all that the engineer holds specially sacred, we might have been living in the Stone Age. A short criticism of some of the papers will therefore serve as a peg whereon to hang an account of the aims and methods of the advocates of Manual Training and of some recent developments in its scope.

The Conference was summoned "to consider the methods of preparation to be adopted by those who contemplate entering the engineering profession in compliance with the conditions laid down by the by-laws for election into the Institution." Now, whilst the justice of the plea for culture and the desir-

ability of a general liberal education cannot be disputed, it is very difficult for the lay mind to understand why the engineer, of all persons, should appear to acquiesce in the claim of classical teachers that they have a monopoly in culture—that no education can be liberal unless the greater part of the time available during the years in which most should be done to promote mental development be devoted to the study of the particular subject they profess. The existence of a tendency—not only on the part of engineers but also on that of the general public—to accept the classicists' claim cannot be questioned; even when the public demands that part of the curriculum of our schools shall be utilitarian, it is conscious the while of a feeling that it is illiberal, if not illegitimate, to do so and only persists in that demand because it realises that life is not long enough for culture to be obtained in the conventional way at the expense of all "utilitarian" knowledge.

One noticeable feature was the general agreement between those who took part in the discussion and the authors of the various papers that specialisation in education was undesirable before a certain age, which on the average was placed at sixteen. The fact of this agreement on a point which is often disputed was practically the only favourable sign obvious in the chaos of special pleading which marked the Conference in the stages referred to.

It will be well to consider some of the papers in detail to see how far the strong feeling against specialisation and the desire for a general liberal education are compatible with acquiescence in the position and claims of the classicist. The criticisms offered are frankly provocative but it is hoped that there is some measure of justification for the attitude they represent.

Dr. Gow's Views

The first paper, "On Literary Education and Engineering," was read by Dr. Gow, Headmaster of Westminster School, an able exponent of the classical position; probably owing to the fact that the subject of his thesis was set for him, his essay was over-weighted with platitudes the truth of which nobody could or would care to deny. One has yet to discover the iconoclast bold (or foolish) enough to say that a "literary education up to certain point" is valueless, even to a crossing-sweeper, if the word "value" be given its widest significance;

until the position held has been assailed, to argue the point is mere waste of time, particularly if there be no definition given of the term "literary." Again, one might ask, if it be now necessary to press the point that the real object of a general education is to learn to employ one's leisure rationally. It is a truism in one sense, although a curious limitation of the object of a general education; here again we are met by the tacit assumption that the words "general education" are synonymous with "classical education" or "education on classical lines." To other points the writer has referred in a letter to the *Times* of July 4. Dr. Gow's address as a whole, however, reflects so peculiarly the limitations of the classical mind that it may be advisable to repeat the criticism offered elsewhere in a more or less modified form. In the first place Dr. Gow states "it is probable that, where Latin is neglected, no very close study is given to any other language." It is permissible perhaps to doubt the legitimacy of the inference; if it be true, it is indeed a terrible indictment of our schools. We may fairly ask what work is done on the modern side during the hours in which the classical side is struggling with the rules and exceptions of Latin and Greek grammar. Surely a more logical deduction—on the assumption that a certain proportion of any time-table is allotted to literary work—is that the neglect or rather the omission as a formal subject of Latin implies that more time is available for other languages, in particular, let us hope, the mother tongue. In fact, Dr. Gow, like the majority of his colleagues, confuses the words classical and literary. There is no doubt that, in the past, the words had the same connotation; surely the assumption is neither necessary nor expedient in these days. No one denies, no one has ever questioned the value of a literary education for any purpose; but it is puerile to argue that study of the classics alone—such study as can be given to them by the average schoolboy—can be regarded as literary education. However far the study of classics may trend in the direction of literary culture—and it would be idle to deny that it does so trend, for this is practically its only claim to the consideration of those interested in education—there is little doubt that the pains and penalties endured by the average boy in acquiring this—to him apparently useless—knowledge, go far to neutralise whatever cultural value such a study may have, apart from the fact that

the subject is introduced to the boy most illogically at an age when he cannot possibly understand or appreciate the grounds for its introduction, thus violating a fundamental hypothesis in the science of education.

Again, it was claimed that "a boy who passes from the classical to the modern side of a school has an immense advantage in inductive science over those who have been educated entirely on modern lines." The justice or injustice of the claim obviously depends on many conditions, not least among which are the standard of judgment set up and the direction in which the superiority claimed was manifested; the writer for one, although a schoolmaster, has grave doubts of the accuracy of the dictum and would even question Dr. Gow's ability to justify his assertion. But if this superiority exist, if it be a real and not an imaginary superiority due to the varying natural abilities of the boys selected as representatives of the two sides, it is probably not so much due to the classical training the boy has had—a thorough training in Latin having caused him to acquire "orderly habits of mind which make inductive science easy"!—as to the fact that the modern side is not so highly developed a member of the body scholastic as the classical side: its functions are little known, may even be misunderstood and are certainly *profoundly modified by the fact that it has inferior material to assimilate and to work with*: in any case the unfortunate youth who chooses or is placed on this side is lost between the Scylla of "commercial subjects" and the Charybdis of a makeshift time-table and is too often in the hands of inferior teachers. It has been argued, with some element of truth, that it does not matter *what* subject be taught but that everything depends on *how* a subject is taught, on the way in which it is presented. Our classical headmasters might consider this hypothesis and before generalising on the relative efficacy of "classical" and "modern" training, should ask themselves: Who is in charge of the modern side? What are its aims and why was it established? Has it been fairly treated in the matter of the selection of pupils or has it been made the "sink" of the school while practically all the boys of ability have been earmarked for the classical side almost in their infancy and their intellect bent into literary channels? Is the curriculum as liberal as is consistent with its aims and the conditions of its establishment and fully adapted to the

needs and capabilities of the pupils or is it frankly catchpenny, a sop to the Cerberus of public opinion? This superiority—"notorious, at least to schoolmasters"—may be more apparent than real and as a working hypothesis is probably invalid, as it neither seems to explain and give meaning to the facts of observation nor does it furnish any logical basis for a "vigorous inductive inference of consequences which are in agreement with reality." In short, we cannot concede the justice of the claim until the one essential condition of accuracy be fulfilled, namely, the careful taking into account of all authentic and *relevant* facts. To adapt freely a passage from the author quoted at the head of this essay, one might perhaps argue in the following way: Education is partly concerned with the acquirement of skill in thinking and it is certain that equal facilities in acquiring this skill are not given by similar arrays of facts; some, *e.g.* lists of rivers, dates, melting points of alloys, lists of exceptions to grammatical rules, atomic weights, etc., *may* be useful items of knowledge but are poor as material for play of thought; others, *e.g.* mathematical data, *may* be useless in themselves while suitable as mental gymnastics. Facts are useful or useless not only as they are true and afford exercise in hard thinking or not but also as they do or do not serve to link up the past and present with the subsequent experiences of the learner. They also derive value from the *way* in which they are acquired and the *manner* in which they are assimilated. Now a good educational system must both serve to convey knowledge and involve the acquirement of mental skill which will be useful not only in this or that occupation but in the cultivation of mental power. Knowledge and skill in using it are useful in this wide sense only when actually utilitarian or when serving as a means to the acquirement of a more extended and directly utilitarian knowledge and skill. Further, the same kind of instruction is not always of the same value, as environments vary. Now consider the claims of supporters of the traditional form of education in the light of the foregoing arguments, bearing in mind the fact that the normal boy, like the normal man, finds it of more absorbing interest to study facts arranged in chains of causation than in any other way and longs to devote his attention to a study that he knows will be useful to him, either at once or in the future.

The remainder of the part of Dr. Gow's paper dealing with

linguistic education was a plea for the study of Latin from the narrowest utilitarian point of view. Briefly, the argument ran thus: The engineer needs to learn the language of a gang of foreign workers in order that he may control them and direct their labours. Whilst their language may be wholly alien to any he has studied, the necessary elements of language must always be the same: the man who, by training, is alive to this fact and on the watch for those elements has a great advantage over another who learns at haphazard. Hypothesis followed by conclusion (understood if not expressed) favourable to a continuance of classical education, to the subordination of literature and content to language study! No one would be so foolish as to deny the value of classics to the student of the Romance languages but surely it is debatable whether the most profound knowledge of Greek and Latin coupled with the oratorical powers of a Gladstone would enable one to acquire easily a knowledge of, let us say, "pidgin English." If the author's premises and deductions are correct, we may ask how it is that Russians and, to a lesser degree, many Orientals have the reputation of being such excellent linguists; they are not worried with "de bello Gallico et hoc genus omne" in their schools! Again, "though it is perhaps not the business of the engineer to make contracts, it is undoubtedly his business to understand them and to give and receive orders accordingly." Conclusion, presumably self-evident as before: waste thirteen or fourteen hours a week for thirty-six or thirty-eight weeks during six or seven years, in order to be able, with doubtful success and many reservations, to understand the archaisms, circumlocutions and elisions of the average legal document! Perhaps we may yield this point. Assuredly classical tradition dies hard but it must be in the last ditch when its advocates are forced to rely on the utilitarian argument backed by such illustrations as these. Over and over again the statement has been made—it was only recently emphasised by the Vice-Principal of Brasenose, the Rev. F. W. Bussell, in a paper in the *Oxford and Cambridge Review*—that the real force of the special line of studies pursued and the classical prejudices rampant in the ancient foundations lies in their freedom from anything commercial and utilitarian.

If then the primary claim which can be made for the study of Latin be that it is "almost an inductive science" and if the

secondary claim be on the score of usefulness, the sooner this subject is deleted from the time-table the better. In studying classics the boy is all too frequently the passive recipient of instruction in which he cannot possibly take a real interest during the early stages and the utility of the study is in no way obvious to him. If there be any foundation at all for the oft-quoted educational principle that no new subject or branch of study should be commenced until the learner realises vividly that it is of importance; if it be in any way true that the individual recapitulates in a more or less contracted and modified form in his own development the history of the development of the race; if the proper function of memory be to inspire the imagination, not to confine it in the armour of hard-and-fast rules: there can be no comparison whatever between the claims of the traditional methods of classical education and the modern methods of scientific and Manual Training; the old ideals of mere study and brain work are obsolescent.

With the closing paragraphs of Dr. Gow's paper on the desirability of a due combination of beauty with utility, on the necessity of cultivating artistic taste, on the advantage of being able to discern "not only what is mathematically possible but also what is artistically impossible," every engineer, every thoughtful man, will be in accord. But surely a love of beauty, a sense of form and appreciation of the fitness of things on the one hand and ignorance of Latin on the other are not mutually exclusive! The critical faculty, pleasure in beauty, delight in reading, sympathy and liberality of thought are emphatically not the concomitants of a classical education alone—as Dr. Gow, by suggestion, would have it—they are far from being its inevitable consequences—as most headmasters would seem to believe—but are derived from every part of a man's training and are compatible with the most profound ignorance of classical learning. And how is it possible to reconcile the Doctor's views as to the desirability of a classical education for engineers, in order to give them culture, taste, artistic feelings, habits of good reading and the like, with his expressed opinion that "the boy who is going to be a first-rate engineer will not take any more literary education after the age of sixteen or thereabouts. Until that age clever boys are almost always equally clever at every subject but at sixteen their tastes begin to be pronounced and their minds cease to be receptive except in one particular

direction"? Has the much-boasted training so little lasting effect as he would seem to indicate? There is no answer to our inquiries but the repetition of old formulæ: discussion is only possible at the risk of excommunication; the present organisation of schools takes for granted the desirability of the rigid maintenance of prevailing orthodoxy. Of course no one would wish to under-estimate in any way the value of atmosphere and of the impress of tradition, the subtle, quiet, indirect effect of ancient buildings and early formulæ, which has helped to build up our independent, alert and self-reliant race; but to confound these indirect aids to development with the narrow formalism of a classical training is indefensible. Great headmasters, like great generals, do not observe precedents; they create them: they are invariably opportunists and realise that the perfection of the curriculum and the time-table are as nothing compared with the importance of developing habits of spontaneity in the minds of their scholars. But it is anomalous to compel development along particular lines in these days of freedom of thought and the chief complaints against formal classical training are that it does not lead to acquisition of the power to observe, to infer and to apply so readily as other possible systems demonstrably do lead; its victims have not that vigorous grasp of fact—and by this one does not mean bare fact without lesson or implication—so desirable in these scientific days, whilst its methods and ideals savour of aristocratic tendencies and lead to the fashioning of all boys into one narrow mould.[1] No one would support the view that intellect is to rule the world to the exclusion of character and that knowledge of fact is more useful than knowledge of men; but while supporters of modern methods in education are not entirely unconscious of the fallacies underlying some of the

[1] Of course not all headmasters are obsessed by classical formalism to the exclusion of modern ideas. The Rev. W. Temple, Headmaster of Repton, while admitting that only about 5 per cent. of the classical side achieve the aim of grasping and realising the whole spirit of the Grecian age and people as revealed in its language and literature, confesses that German might take the place of Greek if studied in the same spirit and with the same motive—with the obvious reservation that whereas an ancient civilisation might be studied throughout its complete cycle of birth, rise, fruition and decay, no modern civilisation could illustrate all these stages. He, however, thinks that a bifurcation of the modern side into its humane and scientific aspects might be of advantage in preventing overcrowding of the curriculum.

proposed developments, the implication that the study of classics *alone* is of value in forming character and is the only "proper study of mankind" must be deprecated. There exist well-defined schemes which do not appeal to self-interest only, which have the teaching of principles as root and motive and on these grounds deserve the same serious consideration as is demanded for traditional methods.

The Rev. Dr. Welldon's Views

Since the preceding paragraphs were written, the fact of the existence of this tendency to exaggerate the importance of the classics while passing over in contemptuous silence the claims of handwork to be considered an important, if not the most valuable, educational medium has received striking confirmation. The Right Reverend Dr. Welldon, in addressing the Educational Science Section of the British Association, said that the subjects of the secondary curriculum must be judged with reference to the final goal of education: if the possible subjects were too many, it would be necessary to strike the balance between utility and culture and so to decide which subjects were indispensable and which might fairly be subordinated and postponed. After pointing out that psychology suggested the adaptation of particular subjects to the awakening of particular powers at appropriate ages, he contended that the basal subjects should form a body of knowledge possessed by all students in common and should be few, suitable to the tender age of school life, practically useful and yet possess the elements of culture. He proceeded to specify religion—as the paramount factor in the discipline of character; mathematics—as possessing the unique merit of showing proof, as distinguishing certainty from probability and as establishing the narrow limits of certainty; natural science—as especially valuable in cultivating the faculty of observation; and languages—to which he awarded the palm as standing in the most intimate relation to human nature. Continuing to discuss the grounds on which languages stood as instruments of education, he stated that English was essential, alike in its literature, language and history, whilst foreign languages were useful and desirable as a mental discipline as well as because they afford means of intercommunication; that classical languages, whilst offering much the same intellectual

benefits as were derivable from modern languages, possessed peculiar advantages in virtue of their remoteness and these advantages were particularly clear when an ancient tongue stood in the relation of Latin to the Romance languages. Arguing thus, he concluded that it was of vital importance that Latin should be one of the subjects constituting the permanent basis of education in all secondary schools and that, apart from the subjects mentioned, viz. religion, English, a modern language such as French, Latin, Mathematics and Natural Science—there was none which could justly claim part in that knowledge which he ventured to describe as the common property of all boys and girls in secondary schools! Here again, in spite of valid and rational hypotheses, in spite of the reference to the well-known fact that psychology suggests the adaptation of particular subjects to the awakening of particular powers at appropriate ages and its peculiar bearing on the supreme importance of handwork, we have the perfectly astounding conclusion that such work may be excluded from the ideal curriculum. With but the barest evidence of the necessity for classical study, the usual conclusion is reached that classics must indisputably form part of the normal curriculum, whilst the educational value of training which involves development of tactic powers is—as we have learnt to expect when a representative of the vested interests speaks—utterly ignored. Of course Dr. Welldon is not entirely ignorant of recent developments, he must know something about the possibilities of the methods connoted by the term "Manual Training." His silence about the subject is significant of the whole attitude of the "knowledge caste." But since the "man in the street" is likely to attach undue weight to the pronouncements of so exalted an individual as the President of a section of the British Association, it is necessary to ventilate the whole question, to demand a reconsideration, a scientific examination of the educational problem and to insist on the fact that there are fatal omissions in the summary of the subjects considered essential to the ideal curriculum by the representatives of the vested interests, by supporters of the classical tradition. It is not that manual training as a school subject is to be regarded as a universal panacea: it, like any other single subject, is only adequate as a centre for a limited number of interests in school. But it is more than probable that its scope, its appeal, is far wider than that of any other

subject. In the past we have overdone "bookishness," we have overestimated the importance of respect for authority and precedent. It is now our duty to demand that those subjects or rather that that form of discipline shall receive attention, which cultivates resourcefulness, which trains in habits of accuracy and truth, which ensures independence of character, as it is our privilege to urge the recognition of the man of action as being of greater worth to the State than the book-worm.

Prof. Thompson's Views

Prof. Silvanus P. Thompson, in his essay, dealt with the extent to which mathematical and scientific subjects should share with other subjects of literate education the attention of schoolboys who intend to enter the engineering profession. The subject again was one indicated by the Institute of Civil Engineers but it must be confessed that the title was misleading. No attempt was made to define the proportions in which the time available should be allotted to the various subjects of the curriculum but it was argued that it was not so important whether the hours per week given to science should be few or many as that precision in expression and the faculty of hard thinking should be cultivated. The lack of definite educational aim exhibited by the majority of schools was criticised and a plea entered for a school leaving examination of a general character in place of the numerous special matriculation and preliminary examinations which are now held. Prof. Thompson also laid emphasis on four recommendations of the Institute in respect of education preparatory to entrance, viz. that:

"1. Specialisation while at school, that is to say, up to about the age of seventeen, is undesirable."

"2. A leaving examination for secondary schools, similar in character to those existing in Scotland and Wales, is desirable throughout the United Kingdom. It is desirable to have a standard such that it could be accepted by the Institute as equivalent to the studentship examination and by universities and colleges as equivalent to a matriculation."

"3. Instruction in mathematics should be on somewhat modified methods."

"4. A general knowledge of elementary physics and chem-

istry or natural philosophy is preferable to the pursuit in detail of some particular department in science."

With the last recommendation he professed hearty agreement but it is surprising that he took occasion to make an attack on the "heuristic" method, which, he stated, "has been absurdly pressed even to the detriment of progress. There can scarcely be anything more dreary than a forced heuristic lesson by a teacher who has neither inspiration nor sympathy. For at school the first object of science teaching should be to evoke interest, not to impart the facts or data of science, still less to systematise their rediscovery. The fossilisation of science teaching is indeed the thing most to be feared." Prof. Thompson must either be misrepresenting wilfully what he knows of the system or he has sadly misunderstood its aims and methods. There is emphatically no more certain way of arousing interest, nothing which appeals so vividly to the imagination, nothing more logical in its development, nothing less likely to lead to over-systematisation, than the method of inquiry. It is legitimate to ask where and how the Professor obtained the information on which he bases his criticism. Surely a teacher who lacks inspiration and is out of sympathy with the subject he teaches or with the methods recommended for its development would make any lesson dreary! Either Prof. Thompson was relying on hearsay evidence and untrustworthy witnesses or he has been singularly unfortunate in the type of pupil with whom he has been brought in contact, for he admits that a bad teacher could make "even electricity as dull and distasteful a subject as the conjugation of irregular verbs." Certain it is that there are teachers who pretend to teach on heuristic lines but who are ignorant of the first underlying principles, who are not aware that it is not necessarily an evidence of disorder that a succession of "wrong" impressions be allowed to register themselves on the brain and become part of its available stock, who do not know that a right conclusion may be drawn from a combination of impressions each of which in itself is inadequate and may be misleading, who forget that overfeeding means sterility and who short-circuit appreciation and realisation with their collateral mental effects in their eagerness—natural perhaps but due to misapprehension of the position—to arrive at some visible result, to teach some definite formula, some bare facts; but

it is more than unjust to misrepresent and condemn a system for the failures really due to badly equipped, half-hearted, unsympathetic exponents.

Prof. Thompson's suggestions for the improvement of science teaching read singularly: "A lesson on the microscope followed by an hour's excitement in examining or making slides; a talk about the telescope and the wonders it reveals, with an actual instrument to see and handle; a chemical chat about oxygen, followed by a few fireworks; all these will be of absorbing interest to intelligent boys of twelve or thirteen. After that a few more systematic lessons will attract, without exhausting the attention of the hearers." Apart from the unscientific view of age limitation made evident by these suggestions, the fallacy involved was pointed out by Spencer: "To *tell* a child this and to *show* it the other is not to teach a child to observe but to make it the recipient of another's observation." No! interests alone are but the frame of mind which looks out for possible experiences, their value is in the leverage they afford, not in the accomplishments they represent; they are not ends in themselves. "We do not trust the *scientific qualities* of one who does not *feel* quite differently about a fact which he himself observed from the way he feels about something he has read or heard." Again: "Among all the habits which science requires us to form, none is more important than the habit of learning when there is no man to teach us, of profiting by our own past errors, of rising on stepping stones of our mistaken selves to correcter judgments."[1] We do not want to teach science but to train in scientific method and there must be a clear line of demarcation between what the individual has observed and what he has learned at secondhand; to learn from authority and to *verify* for oneself being almost as essential as to discover, for no man can find out for himself all he needs to know.[2] The boy must have an observational and experimental[3] acquaintance with the salient and everyday facts of nature and on lines which foster clearness of thought, concentration of attention, skill of hand, precision of eye and

[1] *The Preparation of the Child for Science*, M. E. Boole.

[2] "Life is too short for inventional methods only, a child of to-day . . . is entitled to the heritage of civilisation."—R. E. Hughes, *The Making of Citizens.*

[3] "Experiment is the sole source of truth. It alone can teach us something new; it alone can give us certainty."—Poincaré, *Science and Hypothesis.*

alertness of brain. It is true that interest must be aroused before detailed study is commenced but if the scientific method be pursued, interest continues and increases during investigation or verification so that progress may be made from the simple to the complex, from the indefinite to the definite, from the concrete to the abstract, from the empirical to the rational.

To the remarks on the importance of mechanics and its value as an educational medium, no exception can be taken. "The wise teacher will lead his boys readily from experimental fact to deduced principle and from deduced principle to simple calculations, which can again be verified by experiment." But this reads like a piece of special pleading for the heuristic method and certainly reflects its spirit admirably, though the carping critic might inquire if there be not a danger of systematising the rediscovery of principles, if not of facts, in the teaching of experimental mechanics.

Prof. Thompson also dealt with the recent reforms in mathematical teaching. He appeared to deplore the disappearance of Euclid, gave it as his deliberate opinion that boys are less capable of following a sustained train of thought nowadays than they used to be and stated his belief that their failure was partly due to the new methods used in teaching Geometry. If this be the case, it may perhaps be referred to the fact that we are in the transition stage in which opinion is deliquescent; but as the plan now generally adopted is that of inquiry, the above remarks on the heuristic method apply here and it is hardly necessary to repeat the arguments which have led to the downfall of Euclid. There is no doubt that many capable boys are being trained to think precisely and to express their thoughts clearly, so that some discipline equivalent to that claimed for Euclid's presentation of the facts of Geometry as a chain of connected thought has been found. Since the abstract method formerly pursued—a method designed to lead to a fixed result on a rigidly defined syllabus with a definite unalterable sequence—has given place to concrete treatment leading up to abstract reasoning, the freedom gained is desirable on every educational ground; but this freedom is no valid reason for the neglect of a logical coherent sequence, so that as much, if not more, training in precision of expression can accompany the course in Geometry as has ever been the case. It is all a question of method, of presentation and development combined

with due appreciation of the possibilities of the mother tongue; there is no need for reversion to a formal treatment of logical concatenation quite unsuited for boys.

A General Curriculum

Passing now from destructive to constructive criticism the writer proposes to express his opinion on the curriculum that is desirable for boys until the age is reached at which experts agree a great degree of specialisation is permissible if not necessary. It is with the utmost diffidence that he ventures to take part in a controversy with which specialists alone are considered qualified to deal; the opinions expressed by various authorities are so antagonistic, however, that no compromise seems possible, consequently the organisation adopted is haphazard and of a makeshift nature, and while educational theories and educational schemes generally are in the melting-pot, the incorporation of but a trace of new ideas or of new ingredients may profoundly modify the formulæ which are on the point of settlement. It is a faint hope indeed but, as Locke says in his *Essay on the Human Understanding*, "he that will not stir until he infallibly knows that the business he goes about will succeed will have little to do but sit still and perish."

The normal week in the secondary school contains thirty-two periods—four every morning and two on each of four afternoons; for older boys two successive periods may be allotted to one subject and longer periods are the rule, so that, in their case, the normal week may be said to contain sixteen periods; but for convenience in generalising we may retain the shorter period as unit. Now, as Prof. Armstrong has insisted, there are three necessary forms of discipline—literary work, experimental work and manual work. Other exercises may be expedient but the three mentioned are evidently indispensable. Also whilst admitting its highly controversial nature, one must acknowledge that there is much force in Prof. Stanley Hall's doctrine that it is always wasteful to teach what will never be thought of or practised outside the schoolroom or after school years are ended; though as school, in the wider sense, is never finished, it is difficult to say what subjects are "useful" and when and where the utility of a subject begins or ends. What we have to do to the best of our ability is to choose between various subjects or branches of work and select those which combine the greatest

usefulness with the maximum cultural value. Let us then boldly divide the available time into three parts for the purposes of a rough appropriation and subdivide again as follows:

Literary Work	English (including History, Literature, Grammar, etc., etc.)	6	periods.
	Modern Languages other than English	6	„
Experimental Work	Mathematics on inquisitive lines.	6	„
	Science (Nature Study, Chemistry, Physics, etc.)	6	„
	Geography	2	„
Manual Work	Workshop	4	„
	Drawing School	2	„

The classification is, of course, a very rough one. For example, "science" as a subject should be taught on "workshop" lines, whilst "mathematics" does not come entirely under "experimental" work; but it will serve. For young boys some modification would be necessary in order that time for such subjects as singing and physical drill may be found during the ordinary working hours. An extension of the time allotted to manual work and curtailment of that allotted to the other branches in their case may also be desirable, as experience[1] seems to show that young boys cannot reason much beyond their experience and environment. It cannot be denied that manual work in its broadest form increases the content of experience and widens the environment, whilst there is little doubt that action is the essential factor not only in the formation of character but in the development of intellect.[2]

Canon Glazebrook[3] tells us that languages train the memory, taste, imagination, expression and observation; that history teaches social reasoning about men and institutions; that mathematics educates the faculties for abstract reasoning; and that science develops the concrete reasoning powers and observation. He naturally argues that language should be the staple of our secondary instruction and sets forth cogent reasons for the study of Latin—as the basis of five modern languages, as a mental

[1] *Educational Review*, October 1896 (Amer.).

[2] "If you want to develop the brain effectually you must have recourse to that method by which the evolution of the individual and the race was accomplished. You must do it by the motor function—through the muscles of the hand, which is the chief agent of human activity. Not only is this the best way to reach the intelligence, but it is the best way to build up character."—SIR JOHN COCKBURN.

[3] *Teaching and Organisation.*

gymnastic, as necessary to the intelligent study of English literature; but he forgets or neglects the claims of manual work as a potent agent not only in training the mind—rendering it elastic, supple, adaptable and promoting its growth—but also in arousing in the highest degree the qualities of resourcefulness, inventiveness and initiative; in developing the capacity of meeting change of conditions by new plans; as offering a valid introduction to much scientific work; and as being full of appeal to the interest and imagination. The necessity of introducing and developing such work is now acknowledged by practically every one. Originally introduced into schools merely as affording physical training and as a means of imparting dexterity in the use of tools, its place is still often misunderstood, as one may see it quoted even at the present day as the subject the aim of which is to train the nervous centres of the hands of boys who will not be dependent on manual work as a livelihood; but judged by all standards, manual work appears to include more than any other all the requisites of a true educational discipline; and a discipline which makes a boy plan and think, which makes him recognise his possibilities, which makes him not only willing but eager to tackle difficulties, is worth all the boasted "culture" which a classical education is said to give.

Admitting that such work is desirable, will any one be found bold enough to argue that any subject mentioned in the proposed curriculum should be omitted as either unnecessary or inexpedient or that the time allotted to any one of them is excessive? And if not, one is entitled to ask what subject or subjects must suffer in order that the claims of a dead language may be considered and it may be treated adequately. When the arguments for the retention of Latin are considered, there is hardly one which does not also apply with overwhelming force to the subjects indicated. If it be agreed that specialisation is undesirable below, let us say, the age of fifteen, ought not a time-table without special bias to deserve as much consideration as is demanded for the time-table of the average classical side in which there is nothing but specialisation of the most pronounced type; which, moreover, is only accepted because it is sanctioned by tradition and demanded by the older universities? Interest must be awakened before details can be mastered and it is better to *do* a thing than to learn or read how it *has* been

done;[1] manual work is probably that form adapted to these aims in which it is most easy to give due recognition to the fact that the boy's attitude is synthetic rather than analytic: it readily lends itself to the Socratic plan of teaching *how* to think in order to discover *what* to think; moreover, it is wide in scope, for, as Emerson says, it is the study of the eternal world.

In their book on the higher education of boys in England, Messrs. Norwood and Hope remark that "neither the false Humanism which connotes grammar and formal rhetoric, nor the true Humanism which interprets the noblest thoughts of men, nor yet the Realism which aims at discovering the secrets of nature, can *alone* claim the proud title of a liberal education." The word italicised justifies, if justification be necessary, most of the writer's comments. Such a curriculum as has been sketched would operate against what Canon Glazebrook calls the "baleful predominance of one interest in an unformed mind." The scheme proposed devotes eighteen periods to the more scientific aspects of education, whilst the remaining fourteen are occupied by literary and artistic work—a distribution which seems equitable considering the requirements of the age.[2] The demands of mathematical subjects are large but the point needs no special pleading in view of their acknowledged importance; whilst it is essential that the methods of manual training receive attention in order that mathematical and scientific subjects may be approached with interest and in the proper spirit of inquiry, in order that there may be the atmosphere requisite for their correct presentation and appreciation—this quite apart from all question as to the importance of dexterity of hand and its psychological effect.

It is perhaps worth while considering if a *via media* may be found—if indeed the retention of some classical study be as emphatically necessary as has been argued and the time spent at the secondary school be prolonged sufficiently—by the permission of a certain amount of specialisation at an earlier age than has hitherto been considered desirable (that is, at about seventeen), by the legalisation, as it were, of an *Age of Choice*

[1] Let us not forget that Plato reproached Aristotle with being a reader; that is, one remove from the source of original impressions.

[2] "The whole trend of circumstance is to substitute science for mere rote skill, to demand initiative and an intelligent adaptation to new discoveries and new methods."—H. G. WELLS, *Mankind in the Making*.

when the boy may be expected to have some idea of his future occupation and it might be desirable that the further training he would undergo should exhibit some special bias to that end. The essentials of secondary education which have been postulated apparently leave time for the adequate treatment of the classics only by the omission of more valuable subjects. Let the secondary school then consist of an upper and a lower division and let it be insisted that the curriculum of the latter include only those branches of knowledge which have been defined as forming the *necessary* basis of a liberal education. [The classics as a formal study would be excluded; but obviously there is no reason for neglecting entirely certain aspects of classical work: *references* would be unavoidable in literature, history and grammar; it is merely its formal and excessive consideration that would be out of place.] Then, at the average age of, let us say, fifteen—by which time, presumably, there would be a certain foundation of necessary experience in the boy's mind and he would therefore be capable of appreciating an extension of knowledge beyond his immediate requirements; by which time, in fact, he begins to exhibit definite tastes and preferences—let us permit a minimum of specialisation which may and should gradually increase during the remainder of the period devoted to secondary education. The upper division of the school might, in short, consist of classical, mathematical and scientific, commercial or mercantile and other "sides." This is equivalent to allowing the upper school to divide into sections each of which should exhibit a definite bias whilst not neglecting the claims of general education and development. For example, the classical side would require special attention to literary work, to the study of the classics in particular, whilst insisting on a certain minimum of mathematics, science, modern languages and manual work; the work of the mathematical and scientific side would include a certain minimum of classical study—if such were thought desirable and advisable from the point of view of general culture and necessary for the proper development of English work—of modern languages and of manual training, whilst the teaching of scientific method and the applications of mathematics and science would receive particular attention; the commercial or mercantile side would specialise on modern language and perhaps, more doubtfully, the more technical aspects of the necessities of commerce while

not ignoring general culture—some classics, some mathematics and science, some manual work. At Frankfort the study of Latin is postponed until the age of twelve. Perhaps a further postponement until the suggested "age of choice" may meet the difficulty of an overcrowded time-table and the consequent omission of essential disciplines, although there is undoubtedly a danger that the proposed splitting up of the curriculum at that age may mean excessive concentration on the special branches of knowledge selected by each department, with the development of "one-sidedness" in consequence. This source of weakness in the scheme could, however, easily be neutralised by adequate supervision on the part of the responsible authorities and a plan which appears, in some degree at least, to evercome the acknowledged difficulties of this problem of education is worth careful consideration. Summarising these demands we have the following detailed time-tables, which are of course but tentative suggestions and which probably do not approximate at all closely to those forms which will eventually crystallise and which experience alone can determine and dictate.

1. For boys in secondary schools from the age of entry until the average age of fifteen:

Literary Work	English Literature	2	10 periods.
	„ History	2	
	„ Grammar	1	
	Divinity	1	
	French	4	
Scientific Work	Mathematics	5?	19 periods, the distribution of which will vary slightly at different ages.
	Experimental Science	6?	
Manual Work	Workshop	4?	
	Drawing School	2	
	Music	1	3 periods.
	Physical Drill	2	
	Total		32 periods.

2. For boys in secondary schools from the age of fifteen until the leaving age:

	Classical Side.	Science Side.	Commercial Side.
Classics	15 periods	6 periods	8 periods
English			
Modern Languages . .	4 „	4 „	12 „
Mathematics and Science .	8 „	16 „	8 „
Manual Work . . .	3 „	6 „	2 „
Other specialised subjects .	2 „		2 „

A Scheme of Manual Work

The need for Manual Instruction being admitted and its place in the curriculum being assured, it is desirable that a brief account of methods be given in support of the claims which have been made. It must be remembered that these claims are based on the assumption that manual work is admitted to the curriculum on the same equal footing as other school subjects, not taught under conditions which minimise or destroy its importance nor regarded as suitable only for those of weak intelligence. These conditions are important but it is impossible to treat them adequately in this paper, as the discussion would involve some consideration of the teachers who are available—their knowledge of boy character, their ability to teach, their acquaintance with the subject and its possibilities as an educational medium; in fact, the whole question of the teacher's training crops up. Although difficulties in connection with discipline do not often occur in the workshop, it is evident that a teacher of manual work should be one whose personality is likely to influence his pupils for good; a knowledge of "method" is of advantage and it is desirable, nay, necessary, that he be a skilled workman. Nothing is more fatal than the idea that any one with belief in the work can acquire enough of the technique in a single holiday to become an efficient teacher, even in a preparatory school—the opinion expressed in the report of the curriculum committee issued by the Headmasters' Conference in 1910. The narrowness of the view of the aims and possibilities of hand work such an expression of opinion reveals is only too obvious. How can it be justified when coupled with such cogent reasons for regarding the work as important as the following?—

"It gives . . . in a concrete shape that clear distinction between a task rightly done and one which only approximates to being right, which is lacking in all language work, except, perhaps, in the earlier stages of Latin verses." "Also it is certified by physiologists that the training of the hands in delicate work is a direct, efficacious and quite wholesome way of stimulating the brain. Lastly it is the work which encourages a pupil to solve his own problems without relying on external help."

A work with such possibilities as are acknowledged here is to be jeopardised by being placed in charge of a man who cannot possibly have a more than superficial knowledge of its require-

ments and its technique! What would be thought of a head-master who placed his preparatory classical work in the hands of an expert teacher of science or mathematics or of a modern language specialist, who without knowledge of Latin teaching but that given by general reading, hearsay and theory, attempted to acquire the art in one holiday? And, *mutatis mutandis*, the cases are hardly parallel. Such a teacher as the Conference had in view may conceivably appreciate the subject because of its moral effect on other school work and its indirect influence on expression; in its pedagogic aspect from the self-reliance it fosters, the artistic training it gives and the accuracy it demands; in its political, ethical and economic significance, in short; but unless this appreciation be associated with the power to *create*, to *do*, its importance in conferring teaching qualifications is all but neutralised and is robbed of much of its significance. A *modus vivendi* may be reached by appointing a specialist qualified to organise the work, who may delegate certain duties to instructors capable of performing them. But the whole problem bristles with difficulties and its consideration must be deferred for the present.

Manual Work is a comparatively recent addition to the curriculum but it is not new. Comenius (1592—1671) taught that knowledge should be derived at first hand through the senses and his ideas, developed at intervals by Locke (1632—1704), Rousseau (1712—1778), Pestalozzi (1746—1827), Herbart (1776—1841), Froebel (1782—1852), Spencer and others, have more recently been made the subject of a critical examination by Dr. Dewey and Prof. Hall in America and by such English authorities as Quick, Magnus and others. It is noteworthy that it was made compulsory in France in 1882. Recent as the subject is, however, there has been a tendency to stereotype the teaching and to allow it to fall into a groove; it has been allowed to degenerate owing to the surbordination of method and spirit to formal scheme, of content to form, of root idea to typical "exercise" and to that "fatal tendency to dissect large living wholes, which pubescents crave, into elements which they abhor" (Stanley Hall). The primary aim of such training has far too frequently been regarded as the acquisition of mastery over materials and methods and of skill. Whilst its exponents may have believed that the work developed the intelligence, the fact was often neglected in devising and developing their

schemes; the process was everything, the result nothing. It is desirable, however, that the teacher responsible for the work should evolve a scheme—suited to the environment—which takes due account of the "expressional" idea, while mindful of the "technical" principle. As Mr. P. B. Ballard points out in his essay *Handwork as an Educational Medium*, it is desirable that a course should be systematic but it is essential that it be expressional. Above all, latitude must be given: there must be no attempt to impose uniformity on all schools, for only by freedom can those evils be eliminated which arise from the domination of an antiquated syllabus.

An attempt at compromise will be evident in the scheme outlined below, consequently its efficacy must not be judged by visible "results," by completed "models"—although there is no lack of these—so much as by the frame of mind it engenders.

In the case under consideration there is no formal "course" in wood, metal and clay, through the stereotyped set exercises of which every wretched boy must be pushed at the rate of "x" per term: instead an attempt is made to provide occupations of interest, while keeping in view, in a general comprehensive manner, the aims which have been indicated as desirable. Stress is laid on the fact that to direct attention to superficial details obscures principles and leads merely to confusion. Since opinion alters, the formulation of a rigid cast-iron scheme is ridiculous; it would but nullify the conditioning aims. It must be understood that the appended outline is but the momentary crystallisation of a transitional stage in a constantly developing conception.

The youngest boys attending the workshop are on the average eleven years of age. They are started on carton work and clay modelling concurrently. The materials are soft and easily worked so that manipulation does not call for that degree of physical strength which is necessary for much of the work at a later stage. It is easy to suggest a well-graduated series of cardboard models, which are progressively more and more complex and therefore require increasing skill in manipulation of tools as well as of material and, at the same time, afford a convenient basis for much arithmetic and geometry: there is also plenty of scope for originality of treatment and resourcefulness. Adherence to one formal sequence is—as has been

indicated—inadvisable, not only because the teacher loses interest in the stereotyped but because of the danger that in routine work he may be tempted to make too many short cuts for the boy to gain full benefit from the course; also it must be remembered that increasing complexity and difficulty are necessary, since exercises which soon become automatic are of limited value. Neatness, cleanliness and accuracy are essential; their neglect brings its own punishment. At the commencement it is usual to suggest a model or two—in order that unnecessary repetitions of instructions may be avoided; later in the course, occasional suggestions may be advisable but due regard must be paid to the principle that boys should make something in which they *are* interested, not something in which the adult thinks they *should* take interest. The following list of varied objects made by a class of about thirty boys in one term illustrates the progress made and the scope and range of the work:

> Chess-boards, string-boxes, instrument-cases, egg-boxes, square and hexagonal trays and vases of various shapes, napkin-rings, a collar-box, wall-corner pockets, letter-holders, a portfolio, stationery-cases, photograph-frames, pocket-books, geometrical models and the ubiquitous aeroplane.

Much mechanical drawing was done incidentally. In the course of the work, methods of representing and the properties of plans, elevations, developments and the like were investigated and illustrated; these were readily appreciated. As an example, the various steps involved in making, say, a string-box may be taken. A box being shown, a description of its shape is required. It has, in common with other solids, length, breadth and thickness—these being the terms by which the fundamental conception of three mutually perpendicular directions is distinguished. If these are equal, it is called a cube; other somewhat similarly shaped solids are called cuboids: the boys easily recognise the fact that a cuboid of any shape or size can be built up from cubes of one size.

If it cannot be assumed that the boys know how to make a straight line and a right angle by paper-folding or in other ways, this is the place to introduce such concepts. It is not educational to start with the point, line, angle, etc.—mathe-

matical abstractions—and work up to the solid. The fundamental conception in the young mind is that of the solid; points, lines, etc., follow later.

A clear understanding of the difference between shape and size is essential. All cubes are of the same shape; cuboids are not necessarily of the same shape, although they may be built from cubes and are alike in certain particulars. Methods of making a cubical box are now investigated. If necessary, a model may be cut and the boys allowed to see its net or development. Considerations of the size and shape of one face and repetition of the drawing in convenient positions give the required development. When the box is too large to be drawn full size in the books supplied, the question of scale drawing is raised. Next comes the problem of making the box: is it to have overlaps for glueing or are the edges to be bound with tape? If the first plan is chosen, there is room for thought in deciding where the overlaps are required. Then comes the lid, the drawing and construction of which should be possible without further assistance. Some boys at least will allow for the thickness of the material (it is astonishing how much and how readily boys understand if one refrains from explanation); others may be allowed to complete the lid and discover for themselves what is wrong: they are not likely to repeat the error.[1] The writer holds that the construction of proper working drawings should be an integral part of the scheme. Such work cannot be commenced too early; boys of the specified age soon learn to understand the use of plan and elevation as readily as they do that of developments. For some models plan and elevation might be given and the development required: for others the process might be reversed. A difficulty is that of finding a rational method of approaching the problem of drawing parallels and perpendiculars. If it be granted that the straight line is usually copied and not generated from first principles in each case, one does not see why the question of parallels and perpendiculars should not be

[1] With reference to this, it may be pointed out that methods may be grouped roughly under two headings: the didactic or do as you are told and the heuristic, research or run and find out. The first may perhaps give good results but means bad training: the second may give poor results but the resulting training is excellent. Taking into consideration the time element, a judicious mingling of both systems is probably most satisfactory but guidance must be reduced to a minimum.

treated in like spirit and the beginners shown or led to discover how to use the set squares. Of course many experiments on direction and other geometrical concepts are necessary but the necessity for such experiments lies in the fact that the boys understand the use of drawings when connected with the construction of definite objects: hence there is a valid reason for investigation and the correct atmosphere for its initiation. This cardboard work is most valuable as a preliminary to much formal mathematical work, particularly—as has been shown—to Geometry. A glance at the list of objects made will prove this. Apart from this, the boys acquire skill in manipulating that simplest and most primitive of tools, the knife—a training of the greatest assistance in subsequent developments—and they learn how to "read" a simple drawing.

Clay modelling[1] is designed to train boys to observe clearly by appealing both to sense of touch and to sense of sight: to the former by the contrast of rough and smooth, proportion of relief, etc.; to the latter by judgment of light and shade. Indirectly it teaches self-reliance. By way of exercises on interlacing bands, repetition of leaf forms to produce patterns and mouldings of different sections, it proceeds to work from natural forms: leaves, flowers, fruit—from casts of fine historic ornament and from photographs. To model from nature teaches facts about construction, growth, etc.: to model from historical examples shows how such facts have been used in the production of ornament.

After spending one or two terms on such work as this, boys are prepared—both mentally and physically—to undertake work of a more arduous nature and woodwork is commenced. Not that formal woodwork which has been condemned because it demands the expenditure of much time on mere "exercises" and through reliance on established schemes has degenerated into mere formal routine work; but of a freer, less restricted type which appeals to the boys' innate tendencies to construct and to manipulate and leads naturally—through the actual manufacture of models which do not require much refinement, much detail—to recognition of the necessity for "drudgery" if skill is to be acquired and to gradual appreciation of the reasons underlying this and that formal method, of the steps which led to the invention of certain tools and their importance. Rough

[1] For these notes, the author is indebted to Mr. Rigby of Christ's Hospital.

carpentry, as it is called, provides many objects of the type desired. Packing-cases, stools, troughs, rough sheds, small ladders, etc., are made from rough or ready-planed timber and with such models the strongest possible appeal is made to the constructive instincts, to initiative and resourcefulness, to the feeling for form, to judgment, memory and the powers of observation. They call for the keenest concentration of all the faculties and develop body and mind together. For a detailed account of the methods most recently pursued, the reader is referred to an article in the *School World* in which its development is somewhat exhaustively treated. It will be recognised that the method—broadly speaking—is inventional and as it is evidently capable of almost endless modification and extension there is but little danger of that crystallisation into set form which means inevitable loss of freedom and limitation of scope. The work leads through exercises involving recognised principles, various joints and the use of different tools to all that infinite variety of work for which wood is, *par excellence*, the indispensable material, of which carpentry, joinery and cabinet making are the technical aspects. But with these technical aspects we are not directly concerned. It must not be thought that specialised industrial education is advocated: such a development may be imperative from economic and social considerations but its aims are widely different from ours and its due treatment would exceed the limits assigned to this paper. Such forms of work are merely tapped, to extend the field of operations, on the grounds that if a scheme be too logical, too technical, too formal, there is a danger that natural instincts will be ignored and the fundamental laws of growth violated. As Prof. Hall says, the school must deal with concrete things and the daily objects of the various departments of life, because such dealings stimulate the mental powers and afford them matter to work on, not in order that their study may be an end in itself.

When the boys have been doing woodwork for two or three terms a new field of interest is exploited and metal work is commenced. Such development involves work at the vice with file, hammer and chisel on all kinds of metals—brass, copper, cast iron, wrought iron and steel; forging, the behaviour and working of metals when under the influence of heat, bending, drawing down, upsetting, welding, tempering, with the thousand

and one artifices of the skilled smith; *repoussé* work, so admirable for its combination of demands on knowledge of the behaviour of materials and for artistic treatment; tinplate work, with the knowledge of geometry required, solder, fluxes, etc.; machine work of all kinds, shearing, drilling, shaping, planing, turning, with their interlinked mechanical principles, their invention and development; brass and iron founding, with extensions into the domain of physics and chemistry, which, with pattern-making to serve as a connecting link between wood and metal work, necessitates the power of "seeing in the solid": all these are available and one and all are capable of development on the lines recommended in the case of cardboard modelling and woodwork. They are practical and practicable in every sense and are not only desirable because they are "practical" but—one may insist even at the risk of wearisome repetition—because in the atmosphere of the workshops, where these varied activities are proceeding, there is generated and fostered the frame of mind which demands a clear, vivid and definite apprehension of an underlying theory. Add to these possibilities the additional interests involved in bookbinding, wood-carving, pottery work, building, plumbing, etc., and it will be seen how wide is the field exploited. What other subject can offer such a varied range of interesting and useful occupations? It is, of course, not expected that every boy should become well acquainted with all the branches of work mentioned; nor even, necessarily, that he should dabble in all. Every one, however, should go through the preliminary stages of carton work, clay modelling and elementary woodwork, and an amount of metal work which can be estimated by a glance at the following list of objects made:

> Glass plates, escutcheon plates of various designs, picture-hangers, pipe-racks, hinges, bill-files, etc., involving vice-work in brass; ash-trays, plaques and finger-plates in *repoussé*; straight-edges, latches, pokers, tongs, shovels, bicycle-carriers, punches and chisels, involving work at vice and forge in iron and steel; such articles in tinplate as matchbox-holders, tins, geometrical models, which require soldering; and various designs in wrought iron and copper.

These preliminary stages occupy, roughly, two and a half years; next, about one year should be spent at some other forms of hand work; after this the boy should be allowed to

proceed along the lines which appeal most strongly to him—in most cases, probably, specialised engineering work—but the desire to learn something new should be potent enough to maintain most if not all of the kinds of work quoted in a state of active being, the order indicated not being like the laws of the Medes and Persians. Although the scheme on which the writer relies for these details has been established only two years, its progress has been rapid enough to justify the prophecy that ten years will see his ideals realised and many of the developments proposed firmly established on a sound working basis.

If the case for the desirability of establishing and extending schemes of manual work in schools have been improved in any way by this essay, the claims of the new education will be strengthened and the publication of details warranted.

It is maintained that such curricula as have been outlined afford the basis of a sound *liberal* education and that at seventeen or perhaps earlier a boy who had been to a school where such curricula were imposed would be fit for further and complete specialisation, whether it were for the profession of engineer, for that of man of letters, for a commercial career or for a trade. It should be added that, in the writer's experience, very few boys are quite destitute of fingers; consequently, he believes that there are very few who will not derive benefit from manual training. It is only fitting to conclude with a quotation from one to whom he owes all that is of worth in his outlook on educational affairs, Prof. Armstrong. In his paper on *The Workshop in the School*,[1] this writer says: "I want to see the workshop method introduced—*i.e.* specific tasks set in the form of problems which each child is to work out experimentally, the teacher merely acting as foreman. When experimental work is done in this way, it tends to develop habits of independence, children become observant and thoughtful, their imagination is called into play; if properly led, they become honest workers: instead of becoming parrots, they learn to think for themselves. You cannot give the training in any other way, as you have to develop the practical side of the child to do all this."

[1] *The Teaching of Scientific Method* (Macmillan).

THE BASIC PRINCIPLES OF PROTECTIVE COLOURATION IN THE ANIMAL KINGDOM

By MRS. ARTHUR BELL

Lovers of art on both sides of the Atlantic have long been familiar with the name of Abbott Thayer, whose fine paintings have won him a very high position amongst his contemporaries. But comparatively few, especially in England, know him as the distinguished naturalist who, in the course of many years' close observation in the forests surrounding his New Hampshire home, has discovered what he considers is the basic principle of protective colouration in the animal kingdom.

Speculation on this subject has till recently proceeded on four great assumptions. These are: the principle of the Survival of the Fittest or Natural Selection, the principle of Sexual Selection, the principle of Protection by means of Mimicry and the principle of Protection by means of Concealment. That is to say, it has been taken as proved that different colours and hues have been developed in animals by two main processes, for two main purposes. The first process is the survival of those most able, thanks to peculiarities of colouration, to conceal or disguise themselves, either from their enemies or from their victims and the consequent transmission of these peculiarities to their offspring. Over against this process, however, the followers of Darwin have assumed that another great force was at work, a force making not for but against protective colouration: this is the tendency of females to choose their mates from among the more brilliantly coloured or marked males who compete for them and thus to favour the transmission to succeeding generations of peculiarities selected not because they increase the safety but rather the conspicuousness of their possessor. Hence the existence of highly coloured species is accounted for despite the fact that brilliance of colour has been assumed to be the very reverse of a protection to its wearer. So much for the processes by which colouration is developed The purposes which *protective* colouration serves

have been assumed to be those of enabling the animal to disguise itself as some inanimate object—to pretend to be what it is not; secondly to hide itself from view altogether—to disappear among its surroundings. By either of these two faculties it is able either to escape from its enemies or to approach within striking distance of its victim and thus to survive in the struggle for existence.

Of these four principles, only two are thoroughly accepted and endorsed by Thayer: the whole trend of the evidence which he has collected is towards the most drastic modification, if nothing more, of the remaining two. Natural selection is the process and concealment is the purpose of animal colouration. The spheres both of sexual selection and of protective mimicry are, in comparison, severely restricted. Animal colour schemes are seldom if ever developed by the preference of the female for showy colours and conspicuous forms, seldom if more often used to preserve their possessor from notice by disguising him as another object. Whatever may be the extent to which these latter principles work, the main current of evolution, so Thayer maintains, sets towards the survival or reproduction of the type which most completely conceals itself. Though he is careful, in his most important work,[1] to point out that his method is as far as possible inductive, that he starts with no theories and is only a collector of evidence, still the conclusion which he wishes to be drawn from his data is obvious enough. It is, in a word, that colouration for him means, in the animal kingdom, *concealing* colouration. To accept his evidence is to accept the principle that the colour scheme of the peacock like that of the nightingale, of the zebra like that of the antelope, of the butterfly like that of the caterpillar, has been developed by the strict process of natural selection, as the best fitted to conceal and to obliterate its possessor.

Now, it will be evident that the strength of Thayer's case lies in proving that animals are much better hidden in their natural surroundings than the ordinary observer imagines, that naturalists have been busy accounting for the *conspicuous* colours

[1] *Concealing Coloration in the Animal Kingdom.* An exposition of the laws of disguise through colour and pattern. Being a summary of Abbott H. Thayer's discoveries: by Gerald H. Thayer. With an introductory essay by A. H. Thayer. Illustrated by A. H. Thayer, G. H. Thayer, Richard S. Merriman and others and with photographs. Macmillan & Co., 31*s.* 6*d.* net.

of animals the effectiveness of whose disguise was the very reason for the failure to understand its working. Until you can learn to detect the concealed animal you cannot of course appreciate the laws of its concealment; if you only see the magpie on the road, you cannot understand how it hides itself in the wood.

What then has enabled Thayer to detect a system of obliteration so efficient as to conceal its own methods from practised naturalists? Simply his artist's training. He has approached the problem from the standpoint not of the zoologist but of the painter; and not the least significant feature of his work is the fact that his knowledge of the means by which Art aims at reproducing Nature would seem to have given him a new insight into the working of natural laws. Briefly his conclusion is that in the matter of animal colouration Nature works as does the artist. What the art student has first to learn is to make a drawing or picture on a flat surface look like a solid figure or a landscape. He has to make a flat surface look the reverse of flat. This he is taught to do by three chief means—by drawing, by modelling and by colouring. To produce on flat paper the illusion of a rounded form he first learns to draw the outline of that form; then he shades it in such a way as to give the effect of the play of light and shade on a solid object; lastly he colours it. As a result it appears to stand out in sharp relief against the flat background. Now, Thayer's contention is that in the matter of animal colouration the forces of natural selection are at work to create an optical illusion in precisely the opposite direction. Just as the student cultivates accuracy of drawing and an appreciation of line so as to be able to draw what looks like an animal, so Nature, aiming at creating what does not look like an animal, works as far as possible to obliterate the outline. Just as the student has even to accentuate the play of light and shade on a convex or concave body in his reproduction of it, so in nature the concealment of that body is attained by somehow neutralising or counteracting the same play of light and shade. Just as the student uses colour to give the last touch of individuality to his figures, so in Nature colour serves the opposite purpose: to blur instead of to define, to obliterate instead of to characterise. In a word, in Nature those varieties have survived which most completely fade into their background.

With the governing purpose then of making living forms indistinguishable from their normal background, the development of animal colouration has, according to Thayer, been, so to speak, directed by three great principles, which he calls respectively those of "Countershading," those of the "Averaging of the Background" and those of the "Obliteration of Outline."

In these three principles lies the key to the whole subject. The principle of Countershading is Thayer's cardinal discovery. Though his book is called *Concealing Coloration* it is one of his main contentions that "shading is really more important than colour, because it is primarily an attribute of form, while colour is only secondarily so." "Make the rounded body of a creature look flat and, however it is coloured, it will lose much of its conspicuousness." It is then for this reason that "animals are painted by Nature darkest in those parts which tend to be most lighted by the sky's light and *vice versa*." Thayer insists that the fact that the great majority of living species are shaded more heavily on the back and lightly on the belly is evidence of the working of this principle. The object is to create "a gradation of shading counter to the gradation of shading which light thrown upon the object would produce and having the same note of gradation," with the result that the object appears "perfectly flat, retaining its length and breadth but losing all appearance of thickness." Thus a new emphasis on the distribution of dark and light shading on the animal form is one of the chief results of approaching the problem of colouration from the standpoint of concealment alone.

The principle of Averaging of the Background again gives a new significance to colouration proper. If the illusion of flatness is to have its full effect, the object must be so marked or mottled as to be indistinguishable from its background. Thayer claims that on animals which need the highest degree of inconspicuousness there has been developed "a sort of compound picture of their normal background, a picture seemingly made up of the averaging of innumerable backgrounds." According to Thayer then the principle on which protective colouration proper works is far more subtle than is usually assumed. Thought on this subject has in fact been vitiated by too much bias towards the theory of mimicry. The forces really governing colouration, however, are not working towards making the object resemble some other specific object in the back-

ground—a stone, a tuft of grass or a tree trunk—but rather towards reproducing on the animal's surface the effect of a mixed background—of vegetation, of water, of stone or sand or soil. The animal is not merely coloured; it is painted. Moreover, the way in which dashes or gradations of colours are distributed on the animal's coat is used to help forward the work of countershading—to emphasise the illusion of flatness, to make the animal resemble not an object but a background seen in perspective.

"Patterns on animals' coats" are thus "the utmost that Nature can do" against what Thayer calls the "patent vicissitudes of silhouetting" which follow from the animal moving about out-of-doors against a variety of backgrounds. As we have seen, natural selection works to pick out those species whose colour-scheme is an averaging of their normal backgrounds. But the really protective scheme goes farther still and definitely aims, so to speak, at merging itself into the background so that the outline of the animal is completely obliterated. It is not enough that the living creature should resemble its inanimate background: it must merge with and fade away into it. Thus a new purpose is found by Thayer for the very daring of those patterns, stripes and colour contrasts the developing and fixing of which have so often been traced to sexual selection or again, in some cases of gregarious species, to their efficacy as warning signs or danger signals to others. The peacock's tail, according to Thayer, is not exclusively to please the peahen or the rabbit's to guide its young or warn its fellows. The object of both is to break up the continuous outline of the creature's silhouette; such markings are classified by Thayer as "ruptive" or "secant" for this reason. It is one of his cardinal principles that a scheme of colouration may be at once exceedingly bold and most effective as a disguise. What Thayer does is to extend to a whole variety of similar cases what has long been recognised of the tiger in the jungle: that markings, which against a background of a uniform hue make their possessor exceedingly conspicuous, completely conceal him amid the varied jungle in which he hunts. Anything which breaks up or cuts into sections the surface of the animal's form makes against the possibility of recognising it as what it is.

It follows too from this that Thayer finds a new meaning too

for those appendages and ornaments, such as crests, manes, tail feathers, long ears and plumes, which, seen against an unusual background, seem expressly designed to be conspicuous. The law at work here is, indeed, sometimes very close to that of mimicry. Thayer's contention, however, is that protection is given not by the whole animal disguising itself as an inanimate object but by some, so to speak, outlying portion of it, by its resemblance to a natural object, drawing attention away from what makes the whole animal recognisable, namely, its outline. The two black ears of a white hare may be conspicuous enough against the snow but their very obviousness helps to detract attention from the shadowy body which exactly matches its background. Again, a hanging fringe of feathers may be useful as a substitute for or an adjunct of countershading to conceal the dark shadow on a bird's breast when it is standing erect. Iridescent colours too, by their very brightness, blind the observer to the shape which produces them and blur its outline. In brief, the principle of the obliteration of the outline is that which, of all Thayer's assumptions, is perhaps the most revolutionary in the working out. To grasp it is to feel that the whole problem of colouration has never before been approached from a standpoint which so simply explains so much. It seems to reveal, in the rigorous machinery of the law of natural selection, an instrument of rare efficacy for the preservation and extension of a system of artistic obliteration incomparably delicate and subtle.

Thoroughly to understand that system it must also be remembered that the protective schemes of the animal creation have very seldom been elaborated to hide them from man. It does not follow because the white rump of the antelope is conspicuous to a man on horseback who sees it against the ground, that it is equally so to the wolf who sees it against the sky. Though Thayer considers it incontestable that animals see Nature, broadly speaking, with the same eyes as does man, no one can maintain that they see it from the same angle. Thus, since there is no such thing as "complete intrinsic inconspicuousness," the only way to appreciate an animal's means of concealment is to learn to see it (and this, according to Thayer, is what the ordinary naturalist cannot do) against its normal background and from the position and angle of its most dangerous enemy or favourite prey.

It must not be forgotten, however, that Thayer's work is mainly inductive. What has been said above is necessarily more crudely expressed and more dogmatically asserted than would commend itself to the author of *Concealing Coloration.* What gives its rare note of originality and freshness to the book is the mass of records of first-hand observations and experiences with which it is filled. Thayer's may not be the only theories which could be deduced from his facts but at any rate there can be no doubt of the interest and value of those facts. Take the account he gives how he wrestled with an unbelieving visitor to his house at Monadnock, New Hampshire. This man had rashly asserted that the American coral snake, with its red, black and gold rings, was actually the most conspicuous object in nature. Thayer, with his creed that "there is no such thing as complete intrinsic conspicuousness," was of course pledged to undeceive him. So he made four imitation snakes, one black, one scarlet, one gold and one good facsimile of a coral snake—scarlet, black and gold in one—and put them all in the grass. He then got the sceptic to look at them at a distance of about four yards, whence he saw them all at once except the coral snake, which he was actually unable to detect at his feet. It is to be hoped that he became at once a convert to the efficacy of "countershading" and to the obliterative value of "secant markings."

Here again is a record taken from Thayer's journal, as quoted in his book, of a similar conversion, in which case, however, Thayer was the convert and the place of the prophet was taken by *Botaurus lentiginosus*, the American bittern. This bird, as is well known, has a curious habit of standing in a reed bed absolutely erect with its bill pointing to heaven. This it has always been assumed to do in order to look like a stick or stump such as can occasionally be seen projecting among reeds and thereby to escape recognition as a bird. "If stick mimicry," says Thayer, "were the explanation of the bird's curious trick, what would be the function of the finely-developed sharply-contrasted stripes of light and dark running lengthwise of the head and neck and best shown when the bittern is standing erect with feathers closely depressed? It is plain that these markings cannot help the 'stick' aspect but must rather injure it, inasmuch as a single stick or stem would be of uniform coloration or at most mottled rather than marked with sharp and strong longitudinal stripes.

The true explanation flashed into my mind to-day as I was watching a bittern standing at a distance of about ten feet. The light stripes on the bill were repeated and continued by the light stripes on the side of the head and neck and together they imitated very closely the look of the separate bright reed-stems while the dark stripes pictured reeds in shadow or the shadowed interstices between the stems. The truth of this explanation must be apparent to any one with an eye for these things who watches a bittern standing motionless among reeds." The deduction is of course that those who were clever enough to detect the bittern in the forlorn attempt to look like a stick really only did so because, owing to defects in the background or to the unusual standpoint of his observer, his attempt to disappear into reeds had broken down and his colour scheme was for the moment a failure. When it was succeeding he was practically never seen. But if the bittern and the tiger and the coral snake use stripes as a concealment, why not the zebra; and if the zebra cannot be accused of trying to look like anything, why should it not, in common with many another owner of "conspicuous markings," be really succeeding in disappearing?

This is an implication which Thayer at any rate is prepared to welcome—even to the extent of applying it to humming-birds! Of these Thayer writes that thanks to iridescence "their changeableness of colour often ranges from dull velvety soot-colour to the intensest gleaming of pure red, blue, green, orange or purple, as the case may be, and sometimes several of these bright colours co-exist in the same feathers, showing either separately in different lights or intermixed in one light. But the change from one bright colour to another is less characteristic of humming-birds' iridescence than the change from dull black to keenest brightness. It is in the fullness of this change and the extreme brilliancy of the high-light tints that the supremacy of their coloration lies. Behind the dazzling scintillating blaze of its jewelled head, how can the little round body of a humming-bird be seen?"

"That shifting blaze of red or green or purple light, one instant partly clouded over and in the next flashing out into the sharpest sunlike sparkles, completely eclipses and masks the form and solidity of the body, now veiling it and now

piercing it, so to speak, with all manner of rents and vistas of its brilliant sunlit background, utterly bewildering to the beholder." In fact it becomes apparent that, if this principle be transferred to butterflies, the inventors of similes between those insects and what were supposed to be their human prototypes have been sadly at fault. The aim of the butterfly is not to flaunt itself before admiring spectators but to vanish into its background and not to attract attention but to be inconspicuous. The very caterpillar itself, with its speciality of mimicry, is better material for the rhetoricians of social denunciation than the maligned butterfly which does not even, except in rare cases, pretend to be what it is not.

Elsewhere Thayer works out in even greater detail the concrete results of that averaging of the background which really accounts for so much of the variety and brilliance of animal colouration. Here is a particularly good specimen of subtle analysis and delicacy of description devoted to the American wood duck (*Aix sponsa*). "The general scheme of this beautiful bird's disguisement includes a full potent and obliterative shading from blue black on the back and tail to pure white on the entire underside, shading through sand-colour on the flanks and through chestnut mixed with white on the breast. The throat is also white, ending abruptly against deep velvet bronze and purple on the flanks. Founded on this underlying obliterative shading, which conceals the bird's visible solidity and prepares him for background matching, there is a bright and beautiful system of water-pictures of many kinds, bolder and more vivid than those of any other bird we know (with the possible exception of Steller's eider). For the most part these pictures are of shore and sky reflections, subtly and richly intermingled and composing a great variety of effects. The colours are mainly deep and soft, though rich and liquidly alive with sober iridescence. Their range (excluding the sandy flanks) is from chestnut red glossed over with purple through all degrees of blue to golden green, perfect woodland colours all of them. Most potent of all perhaps are the pictures of reflections on the wood duck's richly crested green and purple head, with its clean-cut shapes and bars of snowy white. These white marks picture bright and sharp reflections of the sky lying on the dark translucent water tinted by vague reflections from the shore. Or again the white and dark marks

together suggest a definite fixed reflection-like picture of a fringe of bushes along the shore, with the bright sky beyond cutting in among their crowns and showing here and there between them lower down. The white in the head and neck and cheeks shows duly bright, while that on the throat from which the higher spots are off-shoots is, in the bird's normal life postures, dull with shadow and belongs mainly to obliterative shading. In the resultant water picture, it renders a duller sky reflection mixed perhaps with underwater effect."

Supplemented as are these wonderful contrivances by ripple sand and water pictures on its flanks, the water duck has become a miracle of elusiveness. "The ripple marks he leaves behind in his wake and those that roll out from his further side are continued and repeated on his obliteratively coloured body and this gives the final touch of perfection to his vanishment and invisibility, so that one may scan a wood-duck-haunted pool at close range and fail to see the ducks that are floating upon it, just as one often looks in vain for the ruffled grouse that is perching motionless in the apple-tree."

Enough perhaps has now been written to show both the originality of Thayer's work and the qualities of style which distinguish the book produced by himself and his son. But a glance at its illustrations is alone enough to demonstrate the reality of this artist's contribution to the advance of science. It is the underlying principle of Darwinism that adaptability to environment is the primary condition of survival. He and his immediate followers have elaborated evidential support for this theory in regard to the organs and structure of animals, till it may be regarded as proved. What all, with very few exceptions, such as Prof. Poulton of Oxford, have hitherto failed to do with any thoroughness is to explore that great field of evidence which consists of the approximation to their environment of the surface colouration of animals. Indeed, by the two hypotheses of sexual selection and protective mimicry, many of them deliberately assumed that such approximation to environment was *not* the governing law of animal colouration. Thayer's contention is that such approximation is, so to speak, actually more literally true of colouration than it is of organic development. Indirectly the prairie over which he roamed is responsible for the development of the horse's hoof. It is *directly* responsible for the colour

and making of his coat, for his hoof was only to run over the prairie, his coat was to fade into it. Thus, if Thayer's doctrine be accepted, it must be taken to give certainly the most subtle and beautiful proof of the theory of natural selection, being that which most clearly shows it at work, matching the creature to its environment. Such matching is surely never so complete, such adaptation is never so delicately poised, as when the creature's visible form actually vanishes into its surroundings.

We are always being accused of living in an age of over-specialisation. The accusation may be just but at least the offence has its compensations. Within the last decade two books on science proper have been published which are in themselves perhaps enough to make us impenitently willing to plead guilty. They are Bergson's *L'Evolution Créatrice* and Thayer's *Concealing Coloration*—the one the work of a French philosopher, the other that of an American artist. Both men are specialists. Yet to read the contribution of either to the theme which has drawn both out of the conventional boundaries of their own studies is to feel that the more profound a man's grasp of his special subject the more piercing may be his insight into that of others. Among the many signs that "Science" (in the narrow sense) is to continue to progress, not the least hopeful is that her problems are continuing to attract not only those who have been trained in her service but also minds whose vigour and acuteness have been tested in other fields.

BASES OF BIO-CHEMICAL INTEREST DERIVED FROM THE PROTEINS

By GEORGE BARGER, D.Sc.

It has long been known that when albuminous substances undergo putrefactive changes a variety of more or less poisonous basic products are formed, often only in very minute proportions; to some of these the term *Ptomaines* has been applied. Of late years much has been done to determine the nature of these products and it is now clear that, as a rule, the bases are closely related to the simple amino- and imino-compounds into which the proteins are resolved by hydrolysis; about sixteen such amino-acid derivatives are now well known and bases have been obtained corresponding to nearly all of them, in most cases, simply by withdrawing carbon dioxide from the amino-acid.

Several of the bases are of particular interest because of their physiological activity and the chapter is one of peculiar importance on this account. Many of the compounds to be described may be formed from any protein exposed to bacterial action. It is not necessary that obvious putrefaction should occur: thus some are formed during the ripening of cheese or by the fermentative changes which necessarily attend the preparation of certain kinds of food. The production of simple, physiologically active bases from proteins, however, is not the outcome of bacterial action alone: probably they are also formed, by natural processes, in the tissues of animals and it is in some such way that we can picture the production taking place of hormones like secretin, which not improbably are of simple chemical constitution. Later on, in the section dealing with the base β-iminazolylethylamine, attention will be drawn to the close similarity in the action of this base and that of the toxic substance formed in an anaphylactic animal as the consequence of the injection of an innocuous protein.

The general conclusion which can be drawn at present is that the physiologically active substances in the animal organism are simpler than they were at one time supposed to be. We

know that the highly complex proteins are built up from relatively few kinds of simple "bricks" and it is from these that physiologically active substances may be derived by comparatively slight modifications. In the present article an account is given of the discoveries made in this field during recent years as a result of the great extension of our knowledge of protein chemistry within the last two decades.

Amines formed from Amino-Acids by withdrawal of Carbon Dioxide

As almost all the amino-acids obtainable from proteins are well known chemically and have been synthesised, the nature of the bases derived from them is easily inferred. Various laboratory methods are available for the removal of carbon dioxide from the acids. In some cases, as in that of leucine and of tyrosine, it is possible to convert small quantities of the amino-acid into the corresponding amine simply by destructive distillation. Another method, which consists in heating the amino-acid with a strong mineral acid, has been applied to histidine. A third method, which is more complicated but affords better yields, involves the displacement of the amino-group first by halogen and then by hydrogen; subsequently the amide or hydrazide of the acid is broken down either by Hofmann's or by Curtius's method. β-Iminazolylethylamine was first obtained from histidine by the latter process.

Such laboratory methods, however, are much less important than the biological process of removing carbon dioxide by means of putrefactive bacteria. The resolution of proteins to the amino-acid stage but not beyond is effected by "digestive" enzymes, both in the small intestine of animals and in the germinating seeds of plants. The lower plants (yeasts, fungi and especially bacteria) not only bring about the same change but are able to carry the process of degradation still further. Usually nitrogen is first eliminated; thus in putrefaction tyrosine is for the most part converted into *p*-hydroxyphenylpropionic acid,

$$OH.C_6H_4.CH_2.CH(NH_2)COOH \rightarrow OH.C_6H_4.CH_2.CH_2.COOH$$

Yeast converts leucine into the optically inactive and *iso*-leucine into the optically active amylic alcohol present in fusel oil (F. Ehrlich, *Ber.* 1907, **40**, 2538):

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH.CH_2\,CH(NH_2).COOH \rightarrow \begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH.CH_2\,CH_2OH$$

$$\begin{matrix}CH_3\\C_2H_5\end{matrix}\!\!>\!CH.CH(NH_2).COOH \rightarrow \begin{matrix}CH_3\\C_2H_5\end{matrix}\!\!>\!CH.CH_2OH$$

The succinic acid in wine is derived from aspartic acid:

$$\begin{matrix}CH_2.COOH\\|\\CH(NH_2).COOH\end{matrix} \rightarrow \begin{matrix}CH_2.COOH\\|\\CH_2.COOH\end{matrix}$$

There are, however, putrefactive bacteria which—if grown under certain conditions—will first remove the carboxyl group from an amino-acid and convert it into an amine. The first examples of change of this character are due to Ellinger, who showed that putrescine and cadaverine—two of the best known bases formed during putrefaction—may be produced by bacterial action from ornithine and lysine respectively. This change will be described further in a subsequent section. In most putrefaction experiments, the amount of any particular base which is obtainable is very small. Faust, in his investigation of sepsine (*Arch. exp. Path. Pharm.* 1904, **51**, 248), allowed yeast to putrefy by the hundredweight; yet he obtained scarcely enough of the base for analysis. In several experiments Barger and Walpole could only isolate a few decigrams of *p*-hydroxyphenylethylamine from 10 kilos of horse-meat, a quantity which must have been capable of yielding at least 100 grams of tyrosine. Similarly, when pure tyrosine, dissolved in broth, was putrefied by a mixed culture, the yield of the amine, estimated physiologically, was only 0·3–0·5 per cent. In certain cases, however, the yield may be much larger: Ackermann, working with pure histidine and a mixed culture of bacteria, at once obtained nearly 30 per cent. of the corresponding amine; when pure cultures of certain organisms are used, the yield in this latter case may rise to 60 per cent. The cause for the small yields of most experiments may be two-fold: organisms, which attack the amino-acid in the wrong way, for instance by removing nitrogen, may get the upper hand at the outset; moreover, as it is formed, the amine may be decomposed by further bacterial action.

The process of decarboxylation which has been described is apparently not limited to bacteria: "putrefactive" amines are found in fresh ergot and contribute very largely to the activity of this drug.

Paraffinoid Amines derived from Amino-Acids.—Glycine should yield methylamine and alanine (l-aminopropionic acid) ethyl-

amine; however, little is known with certainty as to the occurrence and origin of these two bases.

The two amino-acids next in the series, valine and leucine, both yield amines on putrefaction; the amine corresponding to the former acid, *iso*-butylamine, has been obtained by the action of bacteria on pure racemic valine; the amine corresponding to leucine, *iso*-amylamine, has been isolated from putrid yeast, putrid horse-meat, etc.

The formation of the latter base is represented by the equation,

$$\begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH\,.\,CH_2\,.\,CH(NH_2)\,.\,COOH = \begin{matrix}CH_3\\CH_3\end{matrix}\!\!>\!CH\,.\,CH_2\,.\,CH_2\,.\,NH_2 + CO_2$$

The physiological activity of *iso*-amylamine was first established by Barger and Walpole's identification of the base with Abelous's pressor principle in putrid meat (further referred to below under *p*-hydroxyphenylethylamine). This discovery was of value, as it led to the examination by physiological tests of the homologous fatty amines (Barger and Dale, *J. Physiol.* 1910, **41**, 24). It was found that the simplest amine which had a distinct pressor action (in doses of 1–2 c.c. of a decinormal solution) was butylamine. In ascending the series, the activity increases to hexylamine and then decreases again, a faint pressor action still being observable with tridekylamine, $C_{13}H_{27}NH_2$. On comparing the activity of amines containing the same number of carbon atoms, the members of the *iso*-series were found to be somewhat less active than the corresponding bases with a straight carbon chain.

The conversion of a monamine into a diamine completely changes the physiological activity. Thus when pentamethylenediamine (cadaverine), $NH_2\,.\,CH_2\,.\,CH_2\,.\,CH_2\,.\,CH_2\,.\,CH_2\,.\,NH_2$, is injected intravenously it produces a fall of blood-pressure instead of the rise caused by amylamine, the corresponding monamine.

The two diamines, putrescine (tetramethylenediamine, $NH_2\,.\,CH_2\,.\,CH_2\,.\,CH_2\,.\,NH_2$) and cadaverine (the next higher homologue), originally discovered by Brieger in putrid material of various kinds nearly thirty years ago, were almost the first ptomaines that were thoroughly identified. They were soon identified with synthetic bases of known constitution and have often been met with by later investigators; thus Udranszky and Baumann found them in the urine in cystinuria (*Zeitsch.*

physiol. Chem. 1889, **13**, 562), Winterstein and Thöny found them in cheese and Rieländer obtained them from fresh ergot.

The manner in which putrescine and cadaverine are formed was first established by Ellinger (*Zeitsch. physiol. Chem.* 1900, **29**, 334), who showed that the former is the product of the action of putrefactive bacteria on ornithine or *α*-*δ*-diamino-valeric acid, $NH_2.CH_2.CH_2.CH_2.CH(NH_2).COOH$, the latter being derived in the same way from *α*-*ε*-diamino-caproic acid or lysine. Ellinger thus supplied the first definite examples of the decarboxylation of amino-acids during putrefaction.

Lysine is a diamino-acid formed on hydrolysis of proteins by acids; ornithine is formed by the further degradation of another protein fission product, arginine, which is therefore also to be regarded as a precursor of putrescine,

$$\underset{\text{Arginine}}{NH_2.C(=NH)-NH.CH_2.CH_2.CH_2.CH(NH_2).COOH} \rightarrow$$

$$\underset{\text{Ornithine}}{NH_2.CH_2.CH_2\ CH_2CH(NH_2)COOH}$$

It is of some interest that a base has been obtained recently formed directly from arginine by decarboxylation: this is guanidylbutylamine or agmatine,

$$NH_2.C(:NH).NH.CH_2.CH_2.CH_2.CH_2.NH_2,$$

found by Kossel (*Zeitsch. physiol. Chem.* 1910, **66**, 257) in herring roe, from which much arginine is obtained on hydrolysis. More recently Kutscher and Engeland (*Zentr. Physiol.* 1910, **24**, 479) have obtained the base from ergot. Agmatine has not yet been found as a product of putrefaction; probably it is broken down further into putrescine. It has but little physiological activity.

Benzenoid Amines derived from Amino-Acids.—The physiological activity of the fatty amines which have been described above is comparatively slight; a great increase in activity attends the introduction of certain rings or ring systems into the molecule. Ring structure in itself is not necessarily very effective: for example, cyclohexylamine,

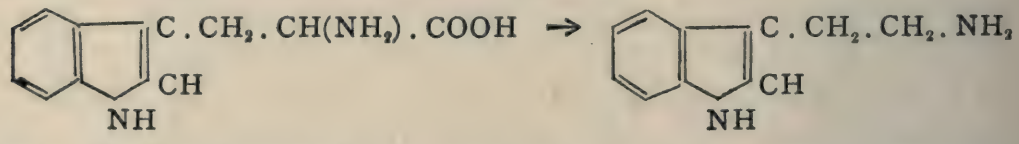
$$CH_2\langle\begin{matrix}CH_2—CH_2\\CH_2—CH_2\end{matrix}\rangle CH.NH_2$$

has a pressor action which is quantitatively very similar to that of the open-chain hexylamine; on the other hand, the presence of a benzenoid group in the side chain is productive of more

effect, as shown by the greater pressor activity of phenylethylamine. The heterocyclic ring in β-iminazolylethylamine confers great physiological activity on the molecule; the activity of many complex plant alkaloids doubtless may be referred to the same cause.

Phenylethylamine, $C_6H_5.CH_2.CH_2.NH_2$, is the simplest benzenoid putrefaction base and is of some historical interest as being the first ptomaine that was isolated in the pure state and analysed. Nencki, in 1876, obtained it by allowing a mixture of gelatin with ox-pancreas to putrefy. At first he regarded it as collidine or trimethylpyridine, with which it is isomeric; subsequently he established its true constitution and considered that it was formed from the amino-acid phenylalanine, $C_6H_5.CH_2.CH(NH_2).COOH$, which has since been found in many proteins.

The other known aromatic amino-acids are tyrosine and tryptophane. The former yields *p*-hydroxyphenylethylamine which will be considered in a separate section on account of its physiological activity. The amine into which tryptophane is converted by the withdrawal of carbon dioxide, 3-*β-Aminoethylindole*, has only been obtained quite recently—



This base is formed from tryptophane by putrefactive bacteria (Ewins and Laidlaw, *Proc. Chem. Soc.* 1910, **26**, 343) and has been synthesised by Ewins (*Journ. Chem. Soc.* 1911, **99**, 270) from γ-aminobutyrylacetal and phenylhydrazine. The base has about the same pressor activity as *p*-hydroxyphenylethylamine but the action is of a somewhat different type.

p-Hydroxyphenylethylamine.—This base was first prepared without the aid of micro-organisms, simply by heating tyrosine (Schmitt and Nasse, *Annalen*, 1865, **133**, 214),

$$OH.C_6H_4.CH_2.CH(NH_2).COOH = OH.C_6H_4.CH_2.CH_2.NH_2 + CO_2$$

Only a small amount was obtained and the base remained almost unknown during nearly forty years. It was then found to occur naturally by quite a number of investigators. Emerson (*Beitr. Chem. Physiol. Path.* 1902, **1**, 501) separated it from the products of the autodigestion of pancreas and Langstein (*ibid.*

1902, **1**, 507) from those of prolonged peptic digestion. It may be doubted whether the experiments were carried on under sterile conditions. The base was found also in Cheddar cheese by Van Slyke and Hart (*Amer. Chem. J.* 1903, **30**, 8) and in Emmenthaler cheese by Winterstein and Künz (*Zeitsch. physiol. Chem.* 1909, **59**, 138); the writer has extracted relatively large quantities from Gruyère cheese. Gautier (*Bull. Soc. Chem.* 1906 [iii.], **35**, 1195) obtained the base from putrid cod-livers, the source of the old-fashioned dark-coloured cod-liver oil.

In all these investigations, however, the marked physiological activity of *p*-hydroxyphenylethylamine was overlooked, probably because it was not expected that the inert amino-acid tyrosine would give rise to a physiologically active substance when deprived of the carboxyl group. Physiological activity was first associated with the base when Barger and Walpole (*Journ. Physiol.* 1909, **38**, 343) recognised that it was the chief pressor principle in extracts of putrid meat.

It may be of interest to mention the steps which led to the latter investigation. In tracing the manner in which adrenaline (adrenine)—the pressor principle characteristic of the adrenal gland—is formed, the French physiologist Abelous and his pupils found that the activity of the gland increased on incubation with meat; subsequently (*C. R. Soc. de Biol.* 1906, **1**, pp. 463, 530) they observed that an extract of horse-meat which had been incubated and putrefied by itself raised arterial blood pressure when injected intravenously. They obtained an impure hydrochloride of a base which was soluble in chloroform which exercised a marked pressor action; they did not identify the base. Subsequently, Dixon and Taylor (*Brit. Med. Journ.* 1907, ii. p. 1150), influenced doubtless by the discovery of secretin and other hormones—investigated the action of extracts of human placenta and found that such extracts injected intravenously caused a rise of blood pressure and also contraction of the pregnant uterus. Then it was found by Rosenheim, who attempted to isolate this active principle of the placenta, that only the more or less putrefied placenta gave active extracts, those from the fresh or merely autolysed organ being inactive; Rosenheim was thus led to the view that a substance was present in the putrid placenta identical with that obtained by Abelous and his collaborators from putrid meat; the identification of this substance only became possible after Barger and Walpole had

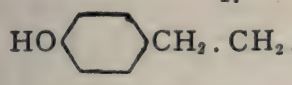
shown that the action observed by Abelous was exercised both by *iso*-amylamine and *p*-hydroxyphenylethylamine. The former of these bases is soluble in chloroform and yields Abelous's crystalline hydrochloride; it is probably also present in placenta; it is relatively abundant in extracts of putrid meat but it is not very active. The latter base is much less abundant in putrid meat and is insoluble in chloroform; whilst it has an action resembling that of *iso*-amylamine, it is much more powerful.

It was, of course, probable that during putrefaction *p*-hydroxyphenylethylamine is formed directly from tyrosine; this surmise was proved to be correct by Barger and Walpole, who grew putrefactive bacteria in a solution of pure tyrosine in broth. The chief physiological effect of *p*-hydroxyphenylethylamine, namely its power of raising arterial blood pressure, had been observed previously in certain aqueous extracts of ergot: Barger and Dale (*Proc. Physiol. Soc.* May 15, 1909) were able to show that the base is also present in extracts of ergot and that it causes the pressor action and contributes to the action on the uterus. It may be that some of the base is only formed during the process of extraction but a certain amount is undoubtedly present in fresh ergot together with several other putrefaction bases.

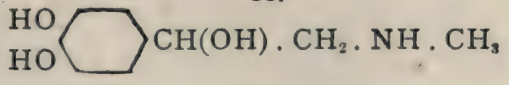
It is very probable that *p*-hydroxyphenylethylamine is formed by putrefaction in the human intestine and it is conceivable that this substance is the cause of the persistent high blood pressure observed in certain persons. *Metschnikoff* has attributed the degeneration of the arteries in old age (arterio-sclerosis) to this high blood pressure, which he considers is a consequence of intestinal putrefaction; hence he has advocated the use of sour milk, in order that the lactic acid bacilli contained in it may drive out the putrefactive bacteria. *Metschnikoff* claims that, by thus limiting intestinal putrefaction, man should be able to attain a greater age than he does at present. Now it is not known whether *p*-hydroxyphenylethylamine can produce arterio-sclerosis but of all putrefaction products hitherto examined this base has the most powerful pressor action; furthermore, arterio-sclerosis can be induced experimentally by repeated injections of another pressor substance, namely adrenaline. If therefore we adopt Metschnikoff's theory, we cannot neglect the possibility that *p*-hydroxyphenylethylamine is a natural poison and one which contributes to the shortening of human life.

p-Hydroxyphenylethylamine (I.) is of interest chiefly because of its relationship to adrenaline (II.):

I.



II.



There is a considerable quantitative difference between the two bases with regard to the pressor action they exert when injected intravenously, 1 milligram of the former base corresponding roughly to $\frac{1}{20}$ milligram of the latter. The nature of the action exercised by the two drugs is very similar, as Dale and Dixon have shown (*Journ. Physiol.* 1909, **39**, p. 25). The effects produced by adrenaline (adrenine) simulate very closely those produced on stimulating nerves of the true sympathetic system, the activity being limited to muscle-fibres and gland-cells innervated by this system. *p*-Hydroxyphenylethylamine (and other monamines, including paraffinoid amines like *iso*-amylamine) also simulate the effects of sympathetic nerves and are therefore also sympathomimetic (Barger and Dale, *Journ. Physiol.* 1910, **41**, 21).

Of the amines derived from amino-acids by loss of carbon dioxide, *p*-hydroxyphenylethylamine is chemically most nearly related to adrenaline and, at the same time, it is most like this base in its action. But many intermediate compounds of the class are conceivable; a large number of these have now been synthesised and their physiological action has been ascertained. The only other substance of this class occurring naturally is the alkaloid hordenine or N-dimethyl-*p*-hydroxyphenylethylamine.

Synthesis of Derivatives of Phenylethylamine. Relationship of Chemical Constitution to Physiological Action

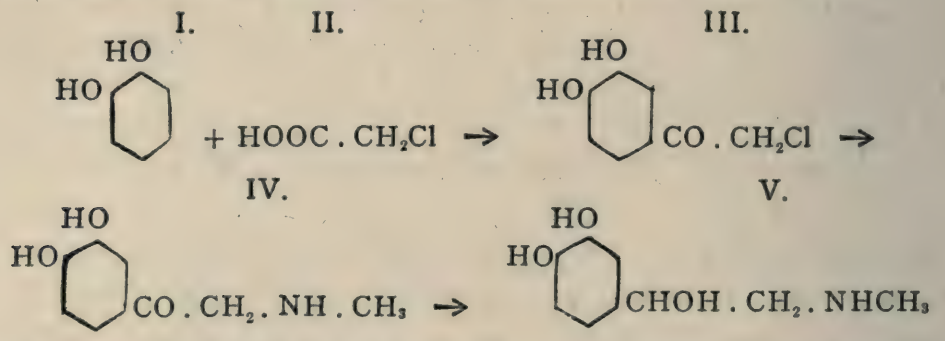
Adrenaline.—This substance, one of the most potent drugs used in therapeutics, occurs in the adrenal gland of mammals to the extent of a few parts per thousand—in such minute amounts, in fact, that the glands of 40,000 bullocks afford only one kilogram of the substance.[1] As little as $\frac{1}{20}$ of a milligram injected intravenously into a cat will produce a very great rise of blood

[1] Quite recently Abel and Ulacht (*Journ. Amer. Med. Assoc.* 1911, **56**, 153) have met with adrenaline to the extent of about 5 per cent. in the secretion of the so-called parolid gland in the skin of a Central American toad (*Bufo agua*).

pressure. The chief use of the substance is in minor surgery: when a solution is applied externally to a mucous membrane, contraction of the arterioles takes place: adrenaline is therefore an excellent styptic. This local action is only exhibited by two or three of its nearest chemical allies, not being shown, for instance, by *p*-hydroxyphenylethylamine.

When the constitution of adrenaline was determined, attempts were made to prepare the substance artificially. The first synthesis was effected by Stolz (*Ber.* 1904, **37**, 4149); the base is now manufactured by his method by the Farbwerke vorm. Meister, Lucius & Brüning under the name of suprarenin (German patents Nos. 152814 and 157300).

The synthesis involves the following changes: Catechol (I.) having been condensed with chloracetic acid (II.)—



the chloroketone (III.) that is produced is acted upon by methylamine and converted into a methylaminoketone (IV.); and this is converted by reduction into the corresponding secondary alcohol (V.) which is racemic adrenaline.

It was found by Cushny, whose observations have been confirmed by Abderhalden and Müller, that the racemic substance obtained in the way described is physiologically less active than the natural lævorotatory substance, the reason being that the *d*-form has only about $\frac{1}{12}$ to $\frac{1}{15}$ of the activity of the *l*-form.[1] This inferior activity of the racemic form might be readily overcome by using more concentrated solutions but the German manufacturers have preferred to resolve their racemic product by means of its acid tartrate (Flächer, *Zeitsch. physiol. Chem.* 1908, **58**, 189).

Various other syntheses of the substance have been carried

[1] A considerable difference in physiological activity between optical antipodes has also been found by Cushny in the case of the alkaloids hyoscyamine and hyoscine. These observations suggest that there must be a chemical combination between the drug and the asymmetric protoplasm of the cell.

out and patented but they are inferior to that outlined above and have no practical value.

p-Hydroxyphenylethylamine.—Since this substance is one of the active principles in ergot, it is prepared synthetically (as "tyramine"). Several methods of producing this and allied substances have been recorded in English and German patents. The oldest synthesis (other than that effected by heating tyrosine) is due to Barger (*Journ. Chem. Soc.* 1909, **95**, 1123) and involves the reduction of *p*-hydroxyphenylacetonitrile, a substance obtained from phenylacetonitrile (benzylcyanide) by successively nitrating, reducing and diazotising:

$$C_6H_5.CH_2.CN \rightarrow NO_2.C_6H_4.CH_2.CN \rightarrow NH_2.C_6H_4.CH_2.CN \rightarrow HO.C_6H_4.CH_2.CN \rightarrow HO.C_6H_4.CH_2.CH_2.NH_2$$

Another method, due to Rosenmund (*Ber.* 1909, **42**, 4778), consists in condensing anisic aldehyde with nitromethane and reducing the compound that is formed; the methyl group is then eliminated by boiling the methoxy-compound with iodhydric acid:

$$CH_3O.C_6H_4.CHO \rightarrow CH_3O.C_6H_4.CH:CH.NO_2 \rightarrow CH_3O.C_6H_4.CH_2.CH_2.NH_2 \rightarrow HO.C_6H_4.CH_2.CH_2.NH_2$$

Hordenine.—This alkaloid, which occurs in barley germs, has also to some extent been applied therapeutically. It has been synthesised by Barger (*Journ. Chem. Soc.* 1909, **95**, 2193) by acting on phenylethylalcohol (a commercial product) with phosphorus pentachloride and heating the chloride that is found with dimethylamine; the base thus produced is converted into hordenine by nitrating it, then reducing, diazotising, etc.:

$$C_6H_5.CH_2.CH_2OH \rightarrow C_6H_5.CH_2.CH_2.Cl \rightarrow C_6H_5.CH_2.CH_2.N(CH_3)_2 \rightarrow NO_2.C_6H_4.CH_2.CH_2.N(CH_3)_2 \rightarrow NH_2.C_6H_4.CH_2.CH_2\ N(CH_3)_2 \rightarrow HO.C_6H_4.CH_2.CH_2.N(CH_3)_2$$

Rosenmund (*Ber.* 1910, **43**, 306) has arrived at the same result by methylating *p*-methoxyphenylethylamine prepared as described above and then removing the methyl group:

$$CH_3O.C_6H_4.CH_2.CH_2.NH_2 \rightarrow CH_3O.C_6H_4.CH_2.CH_2.N(CH_3)_2 \rightarrow HO.C_6H_4.CH_2.CH_2.N(CH_3)_2$$

The methylation is somewhat difficult to effect, on account of the variety of products it affords; the direct methylation of *p*-hydroxyphenylethylamine to hordenine by means of methyl-iodide appears to be impossible, as hordenine methiodide is formed; it is said, however, that hordenine may be obtained by the use of methyl chloride as methylating agent.

Other Substances.—In addition to the three above-mentioned

substances, which all occur naturally, a large number of allied bases have been prepared by Stolz (*Ber.* 1904, **37**, 4149), Dakin (*Proc. Roy. Soc.* B, 1905, **76**, 491), Barger and Jowett (*Journ. Chem. Soc.* 1905, **87**, 970), Pauly and Neukam (*Ber.* 1908, **41**, 4151), Böttcher (*Ber.* 1909, **42**, 253), Tutin, Caton and Hann (*Journ. Chem. Soc.* 1909, **95**, 2113), Johnson and Guest (*Amer. Chem. Journ.* 1909, **42**, 340), Rosenmund (*Ber.* 1909, **42**, 253), Pyman (*Journ. Chem. Soc.* 1910, **97**, 268), Mannich and Jacobsohn (*Ber.* 1910, **43**, 189), Mannich (*Arch. Pharm.* 1910, **248**, 127), Walpole (*Journ. Chem. Soc.* 1910, **97**, 941), Barger and Ewins (*ibid.* 1910, **97**, 2253), Rosenmund (*Ber.* 1910, **43**, 3412) and Moore (*Journ. Chem. Soc.* 1911, **99**, 416).

Of the various synthetic bases only three have found a limited application as substitutes for adrenaline, namely ethylaminoacetocatechol $(OH)_2 . C_6H_3 . CO . CH_2 . NH . C_2H_5$, aminoethanolcatechol $(OH)_2 . C_6H_3 . CH(OH) . CH_2 . NH_2$ and methylaminoethylcatechol $(OH)_2 . C_6H_3 . CH_2 . CH_2 . NH . CH_3$; the first two were prepared by Stolz, the latter by Pyman.

This extensive series of more or less closely related compounds, however, is chiefly of interest on account of the information it has given with regard to the question of the relationship of chemical constitution to physiological action. This problem is of great theoretical as well as of practical interest and has led to numerous investigations. Unfortunately, very few generalisations of any value can be deduced from the large number of isolated facts which have been brought to light. Even if we limit ourselves to a single well-defined type of activity, it is almost impossible to predict the physiological effect of any newly synthesised substance. The difficulty is particularly well illustrated by the series of adrenaline-like bases now under discussion. Their physiological action is in all cases of the same type and we might expect that, as by successive steps we approach nearer and nearer to the molecular structure of adrenaline, each step would bring about an increase in physiological activity. This is not so; there is indeed a great augmentation of activity in passing through the series but it is not regular. One and the same modification of structure enhances the activity of some compounds, while it leaves that of others unaffected. Almost any departure from the structure characteristic of adrenaline lowers the activity and we get the impression that natural adrenaline is necessarily also

the most active of the many similarly constituted derivatives which are conceivable. Some of these derivatives have formed the subject of papers by Loewi and Meyer (*Arch. exp. Path. Pharm.* 1905, **53**, 213) and by Dakin (*Proc. Roy. Soc.* B, 1905, **76**, 491) dealing only with bases closely related to adrenaline; more recently a much larger number of bases, differing more widely in constitution, have been dealt with by Barger and Dale (*Journ. Physiol.* 1910, **41**, 19). Some of the conclusions arrived at in their paper are as follows:

(1) An action simulating that of the true sympathetic nervous system is not peculiar to adrenaline but is possessed by a large series of amines, the simplest being primary fatty amines. Such amines are called "sympathomimetic."

(2) Approximation to adrenaline in structure, on the whole, is attended with increasing intensity of sympathomimetic activity and with increasing specificity of the action.

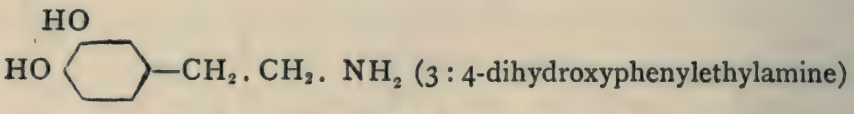
(3) All the substances producing the action in characteristic manner are primary and secondary amines. The quaternary amines corresponding to the benzenoid members of the series have an action closely similar to that of nicotine.

(4) The most suitable carbon skeleton for sympathomimetic activity consists of a benzene ring with a side chain of two carbon atoms the terminal atom in which bears the amino-group. Another preferential condition is the presence of two phenolic hydroxyls in the 3:4 position relatively to the side chain; when these are present, an alcoholic hydroxyl still further intensifies the activity. A phenolic hydroxyl in the 2 position does not increase activity.

It is perhaps worth while to illustrate these conclusions by a few examples. β-Phenylethylamine, $C_6H_5 . CH_2 . CH_2 . NH_2$, one of the bases formed as a product of putrefaction, which has the same carbon skeleton as adrenaline, is five or ten times more active in raising the blood-pressure than the most active fatty amine, thus illustrating the importance of the phenyl group. The introduction of a methyl group or of an alcoholic hydroxyl or of both, which increases the resemblance to adrenaline in structure, as in the base $C_6H_5 . CHOH . CH_2 . NH . CH_3$, does not enhance the physiological activity. More active bases can only be obtained by the introduction of phenolic hydroxyls; a single one of these, in the para position, is already very effective. Again, however, the activity of *p*-hydroxyphenylethylamine

is not increased by the introduction of an alcoholic hydroxyl nor by that of a methylamino group. When two methyl groups are attached to the nitrogen (*i.e.* in hordenine), the pressor activity is much reduced; three methyl groups, as in hordenine methiodide, $HO.C_6H_4.CH_2.CH_2.N(CH_3)_3I$, condition an intense and evanescent pressor activity, which, however, is of quite a different type, resembling that of nicotine.

The activity of *p*-hydroxyphenylethylamine is not much increased by the introduction merely of a second phenolic hydroxyl, whereby it is converted into

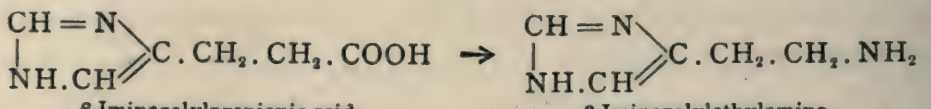


but nevertheless the second phenolic hydroxyl prepares the way, as it were, for further increase in activity, inasmuch as this is greatly increased whether a methyl group be introduced so as to form the secondary base $(OH)_2.C_6H_3.CH_2.CH_2.NH.CH_3$ or an alcoholic hydroxyl group so as to form the compound $(OH)_2.C_6H_3.CH(OH).CH_2.NH_2$; if both hydroxyl and methyl be introduced adrenaline is produced.

Histidine and the Amine derived from it

With the exception of the benzenoid amino-acids, of proline and of oxyproline, histidine is the only amino-acid showing cyclic structure; it contains a glyoxaline ring, which is also present in the alkaloid pilocarpine. When the carboxyl group of histidine is removed, the base that is formed, unlike its parent substance, has great physiological activity and approaches more nearly to the most toxic vegetable alkaloids than does any other amine of its class. This base is:

β-Iminazolylethylamine (4-β-Aminoethylglyoxaline). — Like *p*-hydroxyphenylethylamine, it was obtained synthetically before its occurrence as a product of putrefaction had been observed. In preparing it, Windaus and Vogt (*Ber.* 1907, **40**, 3695) started from iminazolylpropionic acid, which had been obtained previously from histidine and also prepared synthetically; this acid was converted first into a hydrazide, then into a urethane and finally, by hydrolysis, into the base



Three years later, in the course of his investigations of putrefaction bases, Ackermann (*Zeitsch. physiol. Chem.* 1910, **65**, 504) infected a solution of histidine hydrochloride, to which a little peptone and certain salts had been added, with a culture from putrid pancreas and obtained after several weeks nearly 30 per cent. of the amount to be expected of the amine. At the same time he isolated a small quantity of the above-mentioned iminazolylpropionic acid.

Neither of the investigations referred to, however, brought to light the remarkable physiological properties of β-iminazolylethylamine, which were not recognised until the base had been identified among the active constituents of ergot by Barger and Dale (*Journ. Chem. Soc.* 1910, **97**, 2592).

It was known that some preparations of ergot had a very powerful effect in causing contraction of the isolated non-pregnant uterus, yet neither of the two active principles which had at that time been isolated (the alkaloid ergotoxine and the putrefaction base *p*-hydroxyphenylethylamine) could produce the effect. Barger and Dale therefore continued their work by searching for a third active principle, adopting as physiological criterion the action on the isolated uterus as used by Kehrer. In carrying out the experiment the uterus of a freshly killed virgin cat or guinea-pig is suspended in a bath of Ringer's solution, at 37°, which is oxygenated by passing in gas from a cylinder. One end of the uterine horn is fastened by means of a fixed platinum hook, whilst the other is suspended by a similar hook and connected by means of a thin wire to a delicate writing lever, which records the contractions of the organ on a slowly revolving drum. Solutions are tested by adding them in small quantities to the bath containing the uterus.

Among the ergot preparations examined in this way by Kehrer, one of the most active was found to be a dialysed extract prepared by a method suggested by Wernich in 1872. In making this "ergotinum dialysatum," Barger and Dale found that the activity of the dialysate gradually increased beyond that originally characteristic of the ergot; the active principle was therefore being formed, although no putrefaction took place obviously. It was also found that commercial extracts of meat (Liebig's) and of yeast produced the same physiological effect to a slight extent and an examination of these by methods devised by Kutscher and others afforded indications that the

active substance might be related to histidine. As a number of decarboxylated amino-acids had already been found in ergot and at least one of these was physiologically active, it was probable that the principle acting on the isolated uterus was iminazolylethylamine; that this was indeed the case was proved by the isolation and analysis of the base from ergot and by comparing it with the base from histidine.

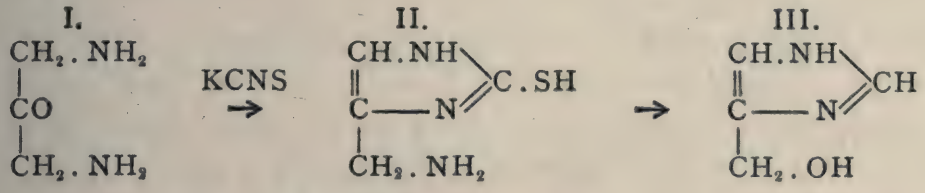
It was in this way that attention was first drawn to the physiological activity of iminazolylethylamine. About the same time Kutscher (*Zentr. Physiol.* 1910, **24**, 163) succeeded in obtaining a compound from ergot which most probably was the base under consideration but he failed to identify it with the putrefaction base from histidine.

Histidine, like all amino-acids, is physiologically inert but the base which is formed by depriving it of the carboxyl group is among the most powerfully active crystalline substances known: one-tenth of a milligram added to a guinea-pig's uterus in a bath of 250 c.c. generally suffices to produce a maximum contraction. The effect of even one part in 250 millions of Ringer's solution is often quite definite. When injected intravenously in small doses the base causes a marked fall of blood-pressure in a dog or cat; less than one milligram will kill a guinea-pig. A peculiar feature of the action is that although the systemic arterioles dilate, those of the lungs contract; this would appear to be the pharmacological justification for the use of ergot in phthisis, as practised by many clinicians.

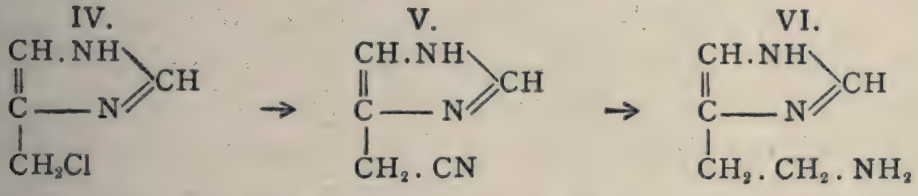
Although iminazolylethylamine is formed during putrefaction and although the base is formed during the lengthy processes of extraction and dialysis in the preparation of some ergot extracts, a certain amount is undoubtedly present in ergot as such. Kutscher obtained his base by extracting ergot with boiling water; Barger and Dale tested an extract of ergot within half an hour of collecting the ergot in the field and found it to be distinctly active.

As the isolation of the base from ergot or from a putrefaction mixture is troublesome, it became desirable to have some other method of preparing the base. It is not possible to obtain the base from histidine by merely heating it but a fair yield may be got by heating with sulphuric acid in sealed tubes (Ewins and Pyman, *Journ. Chem. Soc.* 1911, **99**, 339). A better method is afforded, however, by the synthetic process devised by

Pyman (*ibid.* 1911, **99**, 668). Diaminoacetone (obtained from citric acid) (I.) is heated with one molecular proportion of potassium thiocyanate, according to Gabriel's method; on oxidising the mercaptan (II.) thus produced with nitric acid, the nitrous acid which is formed attacks the amino-group and the thio-amino compound is converted into hydroxymethylglyoxaline (III.):



The hydrochloride of the last-named base may be converted by means of phosphorus pentachloride into chloromethylglyoxaline, a compound which is related to glyoxaline in the same way that benzylchloride is to benzene and is therefore a valuable material for syntheses of glyoxaline derivatives. On converting



the chloride (IV) into the corresponding cyanide (V) and reducing this latter, the desired aminoethylglyoxaline (iminazolylethylamine) (VI) is obtained.

Not only has Pyman been able in this way to obtain the latter base in considerable quantity but by a modification of the method outlined above he has also succeeded in synthesising histidine itself. The synthesis of this important amino-acid had hitherto baffled a number of chemists.

It is probable that small quantities of iminazolylethylamine are somewhat widely distributed in nature. The base has been found in (fermented) Soya beans by Kiyohisa Yoshimura (*Biochem. Zeitsch.* 1910, **28**, 16) and in the mucous membrane of the small intestine of the ox by Barger and Dale. It is most likely therefore the depressor substance in the secretin of Bayliss and Starling, with which its physiological action and solubilities agree. Small quantities apparently are also present in commercial extracts of meat. In these cases, iminazolylethylamine is produced by bacterial action but there are certain

observations which suggest the question whether this base or one closely related to it be not sometimes produced by a fermentative process within the tissues of the animal. These observations are connected with the phenomenon of anaphylaxis. On introducing into an animal a very small quantity of a protein derived from another species, the animal, after the lapse of several days, becomes extremely sensitive to further injections of the protein and this sensitiveness is retained during a very long period. Thus a guinea-pig which has received an injection of a few hundredths of a cubic centimetre of horse serum will, after the lapse of seven to ten days, be killed very rapidly by an interperitoneal or intravenous injection of 5 c.c. of the same serum. On the other hand such an injection would not produce serious results in a guinea-pig which had never been treated with horse serum, so that anaphylaxis had not been produced in it. Now, the remarkable thing is that death caused by anaphylactic shock is closely similar to death produced by an injection of β-iminazolylethylamine, as was pointed out recently by Dale and Laidlaw. Whether an anaphylactic guinea-pig be given a small injection of horse serum or a normal animal an injection of 1 milligram of β-iminazolylethylamine, in either case the animal dies very rapidly from an acute valve-like constriction of the bronchioles which produces asphyxia; after death, the lungs are distended and spongey. Another point of similarity is the extraordinary lowering of the body temperature, which may be as much as 10°.[1] The resemblance of anaphylactic shock to poisoning with the amine is not confined to guinea-pigs, which are very sensitive in this respect. In dogs the action is not so violent, yet very similar in the two cases. The only points of difference are that β-iminazolylethylamine does not cause bleeding of the intestine, nor does it render the blood incoagulable as is the case in anaphylactic shock (and also in poisoning by an injection of peptone). The mechanism of anaphylaxis is still obscure; it has been suggested that the first sensitising injection leads to the production of a ferment,

[1] Quite recently Pfeiffer has shown that the similarity extends even further. Very minute doses of protein produce fever in an anaphylactic animal, instead of a fall of temperature. Now very minute doses of iminazolylethylamine also produce fever. Pfeiffer considers that the base in question will prove of importance in solving the problem of anaphylaxis.

which sets free a very toxic substance from the protein of a subsequent injection. Whether this toxic substance is simply β-iminazolylethylamine, it is as yet impossible to say; the phenomenon appears to be more complicated. Nevertheless, even if we do not go so far as Kraus, who in a recent paper seems to favour this identity, we may well admit that the parallelism between the two kinds of poisoning is very suggestive and such as to support the view that anaphylactic shock is due to some simple toxic fission product of protein.

Ptomaines

The scientific use of this term, introduced by Selmi, is being more and more restricted on account of the ambiguity attached to it. In 1870 and 1871, in the course of a medico-legal examination, Selmi separated from human corpses certain bases which gave the ordinary alkaloidal tests; he was unable to identify the bases with any of the known vegetable alkaloids. Soon afterwards Gautier showed that substances of a basic nature are produced during the putrefaction of proteins. The early investigators of these bases, not unnaturally, were influenced a good deal by previous work on plant alkaloids; the methods used in isolating the animal bases were at first simply modifications of those already in use and were founded on the solubility of the bases in ether, chloroform, amylic alcohol, etc. When Brieger, in 1883, used other methods, such as, for instance, precipitation by means of mercuric chloride, he was at once able to isolate ptomaines which had been overlooked previously on account of their very slight solubility in organic solvents. The use of the older methods devised by Stas, Otto and Dragendorff chiefly led to the isolation of monamines free from oxygen, such as phenylethylamine, which, however, was described erroneously as collidine. The tendency to compare putrefaction bases with plant alkaloids like coniine and nicotine has in some cases led to their conception as pyridine derivatives but as no known protein constituent contains a pyridine nucleus, it is difficult to account for the formation of such bases. If pyridine derivates are indeed formed by the action of bacteria on proteins, the action must be of a very different nature from simple changes such as that of decarboxylation which have been discussed in this article and

must resemble more closely the synthetic processes by which alkaloids are formed in the higher plants.[1]

In the case of two bases, namely those represented by the formulæ $C_7H_{11}N$ (isolated by Gautier and Mourgues from cod-liver oil and regarded by them as hydrolutidine) and $C_8H_{11}N$ (obtained by Oechsner de Coninck from putrid cuttlefish), evidence of heterocyclic structure has been advanced, as it is stated that they are converted by oxidation to pyridinecarboxylic acids. It would be particularly interesting to know the precursors of these bases.

When entire organs or tissues are putrefied, bases may be formed from substances other than the proteins contained in them. A considerable number of bases occur in muscle (meat extract) which may undergo change on putrefaction. Thus kreatine is converted by putrefaction into methylguanidine and possibly also into dimethylguanidine; the purine and pyrimidine derivatives in nucleic acid doubtless also contribute to the formation of bases obtained on putrefaction of the pancreas; the choline of lecithin is set free on putrefaction and in its turn gives rise to trimethylamine. It is thus evident that when an entire organ is putrefied a very complex mixture must be formed and few of the bases can be isolated in sufficient quantity for a thorough investigation; one is often restricted to the analysis of gold or platinum salts.

A further difficulty in the investigation of putrefaction bases is obvious. The working up of large quantities of material is apt to be so unpleasant that it cannot be undertaken in most laboratories. The initial operations can only be carried on out of doors and even after the more volatile malodorous substances, like skatol and mercaptans, have been got rid of, the smell of butyric acid and its homologues clings tenaciously to the investigator.

As a result of this condition of affairs, a large number of bases have been described which have not been chemical entities; sometimes the same base has been described under as many as four or five different names. There is also a good deal of confusion, especially in the minds of the general public, with regard to the physiological effect of ptomaines.

[1] Pyridine bases are not infrequently present in the commercial amylic alcohol used in the extraction of ptomaines. This contamination has not always been guarded against.

They are generally regarded as the toxic constituents of poisonous food, although most cases of so-called ptomaine poisoning are presumably due to infection with a living micro-organism. Some cases, however, are certainly caused by thermostable and very poisonous substances about which little is known. Brieger, who retained the term ptomaine "for the sake of tradition," also applied it to poisonous bases formed in cultures of pathogenic bacteria. On the whole, it is probably best not to speak of ptomaines when putrefaction bases are meant.

Of the principal groups of substances entering into the composition of living organisms, only the proteins can in general form basic substances. In certain cases it is indeed conceivable that the nitrogen may be derived from ammonia but apart from a few doubtful cases, it is certain, or at least probable, that the natural bases are formed from decomposition products of proteins.

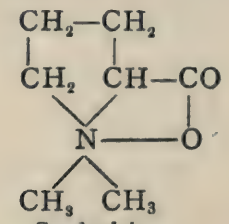
Methylation Bases from Amino-Acids in the Higher Plants

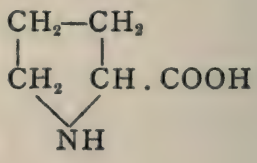
Plants rarely contain primary bases; this applies not only to the true plant alkaloids but also to simpler products derived from proteins, which frequently differ from the corresponding substances of animal origin by having one or more methyl groups. Thus xanthine occurs in animals but theophylline and theobromine (both dimethylxanthine) and caffeine (trimethylxanthine) are found in plants. Adrenaline and sarkosine are among the few substances of animal origin containing a methylamino-group.

It is noteworthy that bacteria and probably also fungi in this respect resemble animals rather than the higher plants. All the putrefaction bases formed by bacteria from amino-acids are primary bases and many are also present in ergot and in other fungi. In vascular plants, methyl derivatives of these bases are found occasionally. The base tetramethylputrescine $(CH_3)_2N.CH_2.CH_2.CH_2.CH_2.N(CH_3)_2$, has been isolated by Willstätter from *Hyoscyamus muticus*; the alkaloid hordenine, isolated by Léger from malt germs (*Compt. rend.* 1906, **142**, 108), in like manner is the N-dimethyl derivative of *p*-hydroxyphenylethylamine, $OH.C_6H_4.CH_2.CH_2.N(CH_3)_2$. Pictet obtained from tobacco the "protoalkaloid" methylpyrrolidine, $CH_3.N=C_4H_8$; pyrrolidine itself has not yet been found as a product of putrefaction but it is probably formed.

In addition to the methylated bases derived from amino-acids, methylated amino-acids may also be present in plants. Thus a monomethyltyrosine (surinamine) is said to occur in several plants. If the nitrogen be completely methylated, an internal salt may be formed by interaction of the amino and carboxyl groups; the so-called betaines are thus produced. The best known of these substances is trimethylglycine or *betaine*, so called because it was found originally in *Beta vulgaris*; it is obtainable in large quantities from the sugar beet and even forms a commercial source of trimethylamine. Owing to the accumulation of methyl groups, betaine is much more basic than glycine and forms a stable hydrochloride.

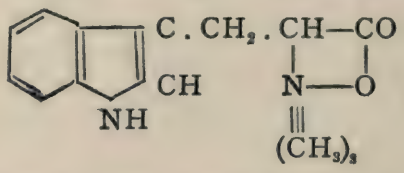
Stachydrine is the betaine of *a*-pyrrolidinecarboxylic acid or proline, a common fission product of proteins:


Stachydrine


Proline

Stachydrine has so far only been found in the tubers of *Stachys tubifera* and in the leaves of *Citrus aurantium* but is perhaps more widely distributed.

A more complicated betaine is that derived from tryptophane,



This substance occurs to the extent of 3 per cent. in the seeds of an Indian tree, *Hypaphorus*, in which it has been found by Greshoff, who described it as an alkaloid, hypaphorine. It has been prepared recently from tryptophane by Van Romburgh (*Proc. Acad. Sci. Amsterdam*, March 25, 1911).

Kutscher has isolated from a commercial mushroom extract a base having the composition of the betaine of histidine. Although the betaines, like the decarboxylated amino-acids, are basic, they are physiologically inert, on account of the presence of a carboxyl group.

THE HISTORY OF AN AGRICULTURAL EXPERIMENT STATION

By A. D. HALL, M.A., F.R.S.

The history of an agricultural experiment station which has been in continuous existence for seventy years must be expected to show considerable changes both in the outlook of those conducting the researches and in the opinion of those concerned with the results that have been achieved.

The Rothamsted Experimental Station dates its formal establishment from 1843. In the period that has since elapsed the whole theory of the nutrition of the plant, the function of manures and the conception of the part played by the soil has taken shape, so that it may not be without interest to trace the successive stages in the development of agricultural science to which the work done at Rothamsted has so largely contributed.

The late Sir John Bennett Lawes was a small Hertfordshire landlord who came into possession of the property at Rothamsted, near Harpenden, when a boy and in 1834 at the age of twenty entered upon the management of his home farm. Both at Oxford—where he had attended the lectures of Dr. Daubeny—and in London—where he had made the acquaintance of Dr. A. T. Thomson, author of the *Pharmacopœia*—he became interested in chemistry and one of his first steps was to attempt the growing of various drug plants upon the farm. His interest in the science of agriculture was perhaps the most excited by the study of de Saussure's *Recherches sur la Végétation* and he began a series of pot experiments with various substances which might possibly be used as fertilisers. It should be remembered that at that time nothing was known of the manurial requirements of a plant; de Saussure, Davy and others had established the nature of the elements usually to be found within the plant but despite the researches of Priestley and Ingenhousz on the assimilation of carbon dioxide the opinion was still general that a plant derived a large part, if not the whole, of the

carbon it contained from the humus in the soil. The researches of Boussingault completed the demonstration on a large scale that the plant drew its carbon from the atmosphere and this point of view was driven home by Liebig's brilliant report to the British Association in 1840.

What we nowadays call artificial manures were practically unknown. From very early times men had learned empirically the value of materials like woollen rags, rape dust, soot and bones but their specific action was unknown and the importations of nitrate of soda and guano and mineral phosphates had not yet begun. Lawes became interested in the utilisation of spent animal charcoal, then a waste product on the market and he discovered that the efficacy as a manure of the phosphate it contained was enormously increased if the material were treated with sulphuric acid, this being also true of other phosphates of a mineral character. His experiments were extended into the fields and having convinced himself of the value of superphosphate as a turnip manure, in 1842 he took out a patent for its manufacture. Liebig in 1840 had recommended that bones should be so treated with acid to increase their solubility but Lawes' experiments had already been successful and his patent, which he confined to the treatment of mineral phosphates, owed nothing to Liebig. With characteristic energy, Lawes set about the manufacture of superphosphate and rapidly built up an industry which provided him with the fortune from which he spent so lavishly in the conduct of the later experiments on his estate.

Lawes' early experiments had thus been of a somewhat unsystematic nature but they had the very practical object of ascertaining how phosphates can best be applied to the nutrition of the turnip crop, which turned out to be particularly dependent upon this element of plant food. Their value was immediately recognised by the farming community, which was then in very active condition and was entering upon its great epoch of development, so that within ten years or so the advantages of using superphosphates had become thoroughly accepted by agriculturists. The importance of the work that Lawes had done was recognised by a public subscription in 1853 which was expended on the erection of a laboratory for the use of the then young experimental station. This latter may be said to have come into existence in

1843, when Lawes secured the co-operation of Dr. J. H. Gilbert, a young chemist who had worked under Liebig at Giessen; the partnership thus entered into continued until Lawes' death in 1900. With the advent of Gilbert, the experiments took definite scientific form and it should be noticed that from the outset they were never designed to demonstrate, still less to advertise, Lawes' manufactures but were laid out to investigate the nutrition of the plant on purely scientific lines. It is interesting to find that though the plots showed the results to be obtained from various sources of nitrogen, from potash and other constituents of the plant, in only one case were they so arranged as to afford a demonstration of the effect of superphosphate or other phosphatic manure.

The question which was most before Lawes and Gilbert when they laid out their first experimental fields, the question moreover which never ceased to occupy their attention, was the sources of the nitrogen of vegetation. Nitrogen is an invariable constituent on the one hand of plants and on the other of the soil but as plants live in an atmosphere of which four-fifths consists of nitrogen, an atmosphere which also contains traces of combined nitrogen in the form of nitrates and ammonia, it naturally became a question of great interest whether the plants derived their elements from the air or from the soil. Liebig very definitely gave his verdict for the atmosphere, maintaining not so much that the plant could bring into combination the free nitrogen gas as that the ammonia which was brought down in the rain-water was quite sufficient for the requirements of all ordinary plants. He therefore advised farmers to restore to the soil the mineral elements normally removed by the crop and considered that there was no necessity to introduce a supply of nitrogen. In forming this opinion, Liebig had been somewhat misled by the exaggerated estimates which then prevailed, owing to imperfect methods of analysis, of the amount of ammonia contained in the rain. Lawes, however, did not agree; his experience as a practical farmer of the increased growth which was produced by some of the fertilisers like dung, which contains more nitrogen than anything else, led him to conclude that the plant must need to draw this element from the soil. The first set of field experiments were therefore laid out to test on all the crops usually grown upon the farm the effects of varying amounts of nitrogenous fertilisers, particularly the salts

of ammonia which were then beginning to be available in commercial quantities; as the results became available he entered the lists against Liebig. An animated controversy followed which lasted as long as Liebig lived, for though the tide of opinion finally settled down against Liebig's point of view, certain outstanding difficulties were never explained until after his death and he has a few faithful adherents even at the present time. The main problem is solved, however.

What the Rothamsted experiments demonstrated was that for the cereals and some of the other farm crops growth up to a certain point was proportional to the supply of nitrogen. The leguminous crops proved to be an exception; they did not respond to nitrogenous fertilisers to any great extent and they proved exceptions in another way, in that it was not possible to maintain their growth year after year upon the same land as with some of the other crops. One of the most unexpected features revealed by the Rothamsted experiments has been the possibility of dispensing to a large extent with a rotation of crops, provided the supply of fertilisers is kept up. One of the cardinal principles in the old conservative system of farming—a principle embodied in the leases under which the land was let—was that two corn crops should never be taken in succession and it would have been considered impossible to grow wheat year after year without any break. Yet the sixty-eighth successive crop of wheat upon the Broadbalk field has this year been harvested and one of the plots has yielded more than five quarters to the acre; *even the plot which has had no manure of any kind since* 1839 *has produced over twelve bushels per acre.* For various practical reasons it is never desirable to attempt such continuous cropping as this but one immediate result of the Rothamsted experiments has been to encourage a much greater freedom of cropping, until to-day it is a regular thing to take two or three corn crops in succession; a few farmers have even adopted the plan of continuous corn-growing, except in the occasional years that have to be devoted to cleaning and restoring the tilth of the soil.

It would be out of place here to discuss in any detail the conclusions which Lawes and Gilbert drew from their field experiments after the results had been confirmed by a few years' repetition but it is enough to say that the accepted theory of the manures appropriate to the different farm crops was estab-

lished by these experiments and that the results have passed into the region of traditional farming practice.

But to return to the nitrogen question. Lawes and Gilbert attacked it in another fashion by repeating in the laboratory certain experiments on the growth of plants in confined spaces supplied with soil and fertilisers of known composition, so as to be able to draw up a balance sheet of the nitrogen in crop and soil as against the original nitrogen in seed and soil. Experiments of this kind are subject to considerable error and no conclusive results had been obtained by several previous investigators: but the Rothamsted experiments, which showed that the nitrogen in the plant had been wholly derived from the soil, were generally taken to demonstrate the fact that the living plant cannot bring into combination the free nitrogen of the atmosphere. As part of the same question, systematic measurements were made of the ammonia brought down in the rain-water, a large gauge measuring one-thousandth part of an acre in area being constructed in order to obtain sufficient rain-water for analysis. During forty years systematic analyses have thus been made month by month, which show that, on the average, the rainfall at Rothamsted brings down about 4 lb. of combined nitrogen per acre per annum, whereas the ordinary crop will remove from 40 to 150 lb. per acre, thus disproving Liebig's opinion that the rainfall provides a supply of ammonia sufficient to maintain the crop without any external assistance from nitrogenous manures.

The leguminous crops provided certain facts inexplicable on this theory of the source of the plant's nitrogen. Several of the Rothamsted records showed that the growth of clover, in addition to yielding a crop containing more than the normal amount of nitrogen, also left the soil richer in nitrogen than it was at the outset; the clue to these facts was finally supplied only in 1887, when Hellriegel and Wilfarth discovered the existence of certain bacteria living "symbiotically" upon the roots of clover and possessing the power of fixing gaseous atmospheric nitrogen. Novel as was this conception, it served to explain the anomalous results which had been recorded at Rothamsted and the later work of Lawes and Gilbert consisted largely in confirming and giving practical shape to this discovery of Hellriegel and Wilfarth. Still later—

after Gilbert's death in fact—other bacteria were discovered living free in the soil which also possess the power of fixing nitrogen and some of the evidence accumulated at Rothamsted served to demonstrate that it is to these later bacteria we must attribute the storing up of nitrogen which has been going on for long ages in some of the rich virgin soils, such as the black lands of Manitoba and Russia. Even the temporary putting down of land to grass and any other farming process which will enrich the soil with carbonaceous matter leads to the fixation of nitrogen, by providing material from which the appropriate bacteria can derive energy. Thus, in a very wide sense, Liebig's opinion is vindicated: the growth of plants, though not of the higher plants, is the only method by which any considerable amount of nitrogen is brought into combination in nature and it is possible to devise a system of farming which will continue to produce good crops without any extraneous sources of nitrogen, provided the land be supplied with lime, phosphoric acid and potash.

With the first twenty years of the field trials, a period which was sufficiently long to eliminate the experimental error and to smooth out the inequalities due to varying seasons, it might have been considered that the original Rothamsted experiments had done their work; it is indeed true that, as regards the immediate effect of the application of the manures to the crops, final conclusions had been reached of which no revision has been necessary. But here comes in one of the chief contributions of Gilbert to the Rothamsted Station. Possessed of a conservative temperament and persuaded that much still remained to be learnt, he held out for the continuance of the experiments without change; as a result, the treatment of the plots has been repeated without variation from at least 1852 until the present day. Thanks to this continuity, the Station now possesses unrivalled material for the study of a number of questions which were hardly appreciated or even suspected at the time the experiments were designed; in fact, there is hardly any portion of the theory of nutrition of the plant which they do not serve to elucidate. It is even to-day impossible to guess in what directions they may not next prove to be of value; as new points of view and lines of research successively open before us, they will doubtless provide material for investigation, either from the past records or the crops actually growing. A

few illustrations of the further developments that have come to light may be given here.

When the Rothamsted experiments were begun, nothing was known as to how such processes as fermentation and decay took place. Liebig and others had attempted certain explanations but that the actions were carried on by living organisms was entirely unsuspected. With the development of knowledge of the actions of the yeast plant in producing alcohol and the early studies of bacterial change carried out by Pasteur, attention began to be directed to the soil as a possible seat of similar actions. In particular it had always been recognised that soil is capable of transforming almost any form of organic compound of nitrogen into nitrates. The higher plants, for example, can only take in nitrogen as nitrate and yet it is a matter of indifference whether we supply a crop growing in ordinary soil with nitrogen as nitrate, ammonia or the various forms present in such material as farmyard manure. Clearly some processes must be at work in the soil transforming these compounds into nitrates. This has proved to be the case. In 1877 Schloesing and Müntz were able to demonstrate that the change was a vital one, as it is suspended when the soil is saturated with chloroform or other antiseptics which will put living organisms out of action by killing them.

The late Robert Warington was then working in the Rothamsted Laboratory and immediately attacked the question in the light of Schloesing and Müntz's announcement. After confirming their results, he succeeded in showing that the action must take place in two stages, each brought about by different organisms, one of which transforms ammonia into nitrite whilst the other completes the oxidation into nitrate. Warington attempted the separation and isolation in the pure stage of the two organisms but used the long and tedious method of dilution and before he had succeeded in purifying his cultures to the extent that would satisfy himself Winogradsky published his very elegant method of isolation, which depended upon the growth of the organisms upon a non-organic solid medium—silica jelly. Warington's investigations on the processes of nitrification were not confined to the laboratory. He showed that the organisms are almost confined to the surface layers of cultivated soils and his demonstration of the

fact that they multiply on stirring up of the soil in response to free aeration and also benefit by a neutral reaction and the presence of mineral plant foods went far to explain the efficacy of many of the ordinary operations of cultivation upon a farm. When, for example, land is bare fallowed during a summer the formation of nitrates is promoted by the aeration of the soil and the increased crop which under normal conditions follows a bare fallow is largely due to the accumulation of nitrates during the previous seasons, provided that they are not washed out of the soil by the winter's rainfall. Many other facts familiar to the practical farmer could be explained in similar fashion, until the rapidity or otherwise of the formation of the nitrates came to be regarded as one of the main causes of the fertility of the soil. The point of view has, however, shifted of late in consequence of some other work which is now going on at Rothamsted. It has been shown that whilst a partial sterilisation of the soil, such as follows from heating it during a couple of hours to a temperature between 60° and 100° C., is accompanied by a complete destruction of the nitrate-making organisms, yet the productive power of the soil is enormously increased. It may even be doubled. This increase in fertility depends upon a great speeding up of the rate at which the nitrogenous residues in the soil are broken down by bacteria to a state of ammonia, though the ammonia is not oxidised to nitrates. Other investigations go to show that plants can feed as freely upon ammonium compounds as upon nitrates, indeed many of the soluble organic nitrogen compounds like the amino-acids appear to be capable of furnishing the plant with the nitrogen it requires. It had been known before that plants under laboratory conditions can draw their nitrogen from ammonium compounds but this was not supposed to take place to any general extent in the field. We have now come to regard nitrification as only the end process, the rapidity of which is determined entirely by the rate at which the other preliminary breaking down of organic nitrogen compounds to ammonia is taking place, this latter being the significant change which limits the supply of nitrogen to the crop. Moreover, soils are to be found in nature in which, owing to their acid condition, nitrification is at a standstill, so that the plants they carry are entirely dependent upon ammonia and other unoxidised sources of nitrogen.

Warington also accumulated several important facts upon another bacterial process going on in the soil which works in an opposite direction and transforms nitrates into nitrogen gas. Somewhat similar organisms again set free nitrogen gas from ammonia and the organic compounds of nitrogen; the whole group of these changes, to which the name of denitrification is sometimes given, involves the impoverishment of the soil by loss of combined nitrogen. Warington investigated the conditions under which these losses are likely to be of practical importance and in this connection the long-continued application of particular manures to the field plots afforded some interesting data. For example, it is found that when nitrogen is supplied as manure to the mangold crop, as much as 78 per cent. is recovered when the nitrogen is put on in the form of nitrate of soda, whereas the recovery from the nitrogen of ammonium salts is only 57 per cent. and from farmyard manure as little as 32 per cent. Nor is the deficit left behind in the soil. On the wheat field, for example, of the nitrogen supplied as farmyard manure year after year until the soil has become exceedingly rich, only about one quarter has been recovered in the crop and another quarter accumulated in the soil, whilst at least half has been lost by bacterial conversion into nitrogen gas. How to tune up this conversion factor and obviate these losses is one of the most important questions before the soil chemist.

Allusion has already been made to the part played by bacteria in the soil in bringing nitrogen into combination and as a consequence of such discoveries the whole point of view of the function of the soil has been changed during the history of the Rothamsted experiments. It is no longer regarded as simply a vehicle for the transference of nutrients to the crop but as an active laboratory, sometimes enriching, sometimes merely circulating but at other times impoverishing the capital embarked in the land; the relative magnitude of these processes will become more and more under control as the conditions under which they operate are better known.

Another important outcome of this vital view of the soil is the question of the conservation of its fertility under different systems of farming. We see from the long-continued trials at Rothamsted that under any scheme of treatment the soil tends to arrive at a state of equilibrium and to yield a crop which only

fluctuates with the seasons. The unmanured wheat plot, for example, has for years given a very constant yield of about twelve bushels to the acre, the annual removal of nitrogen being balanced by recuperative bacterial actions. The dunged plot, on the contrary, has reached another but much higher position of equilibrium, in which the normal annual increments of nitrogen from the manure are disposed of by the accelerated bacterial actions that set nitrogen gas free. Similar wastage takes place when soils rich in residues of previous vegetation, such as the black soils of the prairies or the fens, are put under arable cultivation without recuperative crops. On one of the rotation plots another equilibrium has been reached by which the land is able to yield crops on a level of about twenty-eight bushels of wheat per acre without an extraneous supply of nitrogen, by the growth, once in a rotation of four years, of a crop of clover. An analogous condition must have prevailed under the older system of farming before artificial manures or imported feeding stuffs were available and when the fertility of the farm had to be self-supported. It is necessary that we should be able to draw up a similar balance sheet for various conditions of farming, extensive and intensive, so as to be able to decide, at each level of fertility, how far the necessary nitrogen taken away in the crops or wasted can be supplied by the land itself or must be brought in from without.

Towards the end of his life indeed Lawes was wont to maintain that the chief problem remaining for solution at the Rothamsted Experimental Station was the fate of the soil under the long-continued treatment it had received. We have seen what complexity the question assumed as regards nitrogen. There are, however, other issues. For example, it has been found that repeated applications of ammonium salts set up an acid condition in the soil; as this acid condition suspends the development of bacteria and substitutes for them another race of micro-organisms, light is thrown upon the special difficulties which are encountered in farming land which is acid by nature. Many of the actions going on in acid soils, as for example the accumulation of peat, can be strictly paralleled and will eventually be explained by some of the Rothamsted plots which have been made acid under known conditions. Again, the fate of other manurial constituents besides nitrogen is important. Investigation of the soil and

of the drainage waters of some of the Rothamsted plots which have been continuously receiving soluble compounds of phosphoric acid shows that the phosphoric acid is immediately arrested and accumulates in the surface layers of the soil, where indeed it remains in a condition continuously available for the needs of the crop. Potash compounds, on the contrary, are washed rather deeper into the soil and are not so thoroughly arrested; in both cases the absorption is so complete that the farmer may apply these manures in the autumn and winter and need not fear the loss of whatever is not taken up by the first crop which occupies the land.

Again, the existence in the laboratory of samples of soil taken from the various plots on successive dates in their history led to an investigation of the rate at which carbonate of lime—that indispensable constituent of a fertile soil, is being removed normally by rain and how far this rate is affected by different systems of manuring. In the course of this investigation it became evident that a great number of our cultivated soils owe their present fertility to applications either of quicklime or carbonate of lime that were made at regular intervals during the seventeenth and eighteenth centuries; as this operation has been very generally dispensed with during the last half-century our soils must eventually become impoverished and fall to a lower level of fertility. The lessons thus derived have begun to bear fruit, first of all in the advice given to farmers all over the country and then in the renewed attention that has been paid to the regular use of lime that is everywhere apparent. Many a farmer who is now seeing the benefits of applying lime to his land does not realise that this is the outcome of a purely scientific investigation at Rothamsted, which directed the attention of his immediate advisers to a neglected side of soil management.

Whilst work upon the soil is likely to occupy the attention at Rothamsted for many years to come and to cast light upon many unsuspected difficulties, the crops themselves also provide valuable material for investigation. The question of quality in produce is particularly important to British farmers but is still almost an unexplored field. We know in a general way that particular soils produce crops of a higher quality and we can sometimes roughly correlate this with manurial but more often with physical factors of soil and climate. We do not, however,

know yet what *is* the difference in composition between crops of high and of low quality, still less do we know whether the ordinary criteria by which quality is judged are justified when the material comes to be employed in feeding man or stock. The knowledge that we have of the composition of a crop is only what may be termed a first approximation. We know the amount of nitrogen it contains and can determine what is protein and what is non-protein. We can estimate the chief carbohydrates—starch and sugar—but there are whole groups of minor constituents which have not been discriminated though they must play the all-important part in determining what we call quality. With the improved methods of analysis now at our disposal and particularly with our increased knowledge of the nitrogenous compounds of a non-protein nature, it is possible to attack the composition of crops from a much more refined point of view. We shall also have to work out the presence of a number of substances which exist sometimes in very minute proportions—glucosides, essential oils, alkaloids—which we are beginning to learn may have a most potent effect in the nutrition of animals, because they act as stimuli and starters of many forms of vital action. It is recognised, for example, that the grass of one field may have the power of fattening an animal whereas similar grass growing on an adjoining field, though fed to an unlimited extent, will never bring an animal into condition for the market. Our present methods of analysis fail to discriminate between the two kinds of grass and until they are refined to the pitch of doing so we cannot make much headway with the finer art of feeding cattle and of utilising the land to the best advantage. It is here that the material afforded by the Rothamsted plots becomes of so much value. We have wheat, for example, grown under conditions of normal manuring and on a minimum of plant food, grown again on an altogether distorted and one-sided nutrition, lacking in phosphates for example or overdosed with an excess of nitrogen. When we are certain of the effects which these variations produce in the composition of a given crop we may deduce the causes of a similar shift of composition found in the produce of ordinary farm land elsewhere. Even such a problem as the susceptibility to disease and its causes finds illustrative material on the Rothamsted plots. For example, certain of the mangold plots are in the majority of seasons devastated by the attacks of a

particular fungus late in the year, and though they are equally exposed to the possibility of infection the neighbouring plots remain perfectly healthy. We can only conclude that differences in the nutrition of the plants have produced such differences in the composition of the leaf cells that the fungus can find a medium for its development in one case and not in the other. But as indicated above these differences of composition which lead to such widespread results are extremely elusive and difficult of investigation. Just the same thing is true for the science of feeding animals. In the early days certain fundamental questions of this character were dealt with at Rothamsted. The composition of the carcases of sheep, oxen and pigs at various stages of fatness was established, as were the relations of food to increase and the origin of fat in the animal's frame. But the science of nutrition has now passed out of that early region of first approximations: we know that carbohydrates and fats supply energy which can be stored in known ratios; we know, roughly, what quantities of protein are required to maintain the tissue wastage. What is now required is an attack upon a much more refined class of problem—why an animal in the last stages of fattening responds so well to linseed cake and acquires a special kindly feel and appearance on this food alone; why again cotton cake and barley meal "nick" so well together; why again oats must not be fed to horses until they have been in store for a certain length of time. These are all questions which depend upon much more delicate methods of analysis than those hitherto available, because they deal with constituents untouched in the first approximation to the composition of the material present perhaps only in minute proportions.

The influence of climate and season has naturally received much attention at Rothamsted but it cannot be said that any great advances have yet been made in the way of correlating meteorological statistics with growth. Temperature and water supply are without doubt fundamental factors in determining yield; the difficulty that attends the problem seems to be the collection of data that are really critical. For instance, everybody will agree as to what constitutes a spell of "growing weather" in spring but it is difficult—we may almost say it is impossible—to define such growing weather in terms of the constants usually determined at a meteorological station or to pick out from an inspection of the daily readings when such

a period has taken place in the past. Without doubt the climate chiefly affects a crop by the variations it induces in the water content of the soil, a factor which also depends upon the physical texture of the soil and the treatment to which it is subjected. A certain amount of attention has been given to these points at Rothamsted in the past: for example, determinations have been regularly made of the amount of water percolating through the soil, of the effect of the growth of plants upon the water content at different times of the year and, again, of the changes in the texture of the soil—which determines both its retentiveness and permeability—caused by the long-continued applications of particular manures. It is probable, however, that investigations of this kind will play in the future a more considerable part, as the importance of the physical behaviour of the soil begins to be recognised by scientific men. We have to remember that the labours of the farmer in the spring are almost entirely directed to getting the soil in a particular physical condition; that which he calls "a good seed-bed" and the subsequent growth of the crop will depend far more upon the mechanical texture it has acquired than upon the amount of manure it may receive. When the farmer speaks of the field as having been exhausted by the growth of a crop of wheat he refers far more to the bad mechanical condition it has reached than to the comparatively small withdrawal of plant-food, and most of the objections which from time to time have been taken by farmers, with reason on their side, to particular artificial manures are really due to their secondary effects upon the tilth of the soil. In the importance which is now being given to the physical aspect of the soil we see again how far we have travelled from the early conception of the soil as a mere magazine of ready-formed food which prevailed for some time after the Rothamsted experiments were started. It would be possible to go on indefinitely indicating what work remains to be done and what problems are still unattacked in such a well-worn subject as to the relation of the plant to the soil; sufficient has been said to show how big the task is before the farmer can acquire anything like the control of the soil which the manufacturer, either of metals or of textiles, possesses over his materials. This control is likely to be more necessary to the English farmer than to any other: in no other country is the price of land so high nor the pressure of the population

so great; in no other country, therefore, is there such a necessity for intensive farming and for utilising the soil up to its utmost capacity. That the important farming districts of England do at the present time still show a higher productivity than similar areas in any other country is in no small degree due to the manner in which the farmer has assimilated the lessons of the Rothamsted experiments during the period of expansion and development in all districts which took place from about 1840 to 1860 or 1870. Now that a fresh wave of prosperity seems to be coming to British agriculture, accompanied by an increased intellectual activity and curiosity among the cultivators of the land, we may hope that the later researches at Rothamsted may find both appreciation and translation into practice at the hands of the working farmer.

THE HUMPHREY INTERNAL-COMBUSTION PUMP

By HUGH DE P. BIRKETT, B.Sc.

The raising of large quantities of water with a minimum of fuel consumption has constituted an important problem ever since the introduction of the steam engine, as witness the Cornish Pump and the many modifications of steam pumping machinery since made. The record for fuel consumption per horse-power has long been held by steam pumping plant; it was only on the introduction of the steam turbine that superior results were obtained and the electric station beat the pumping station in economising fuel. The improvements effected recently in internal-combustion engines have made still greater economy possible and such engines now hold the record. There are few types of pumps, however, which can be coupled directly to gas engines with advantage and in any case the use of such a combination is far from ideal.

Granted that the principle of internal combustion leads to the maximum economy, the problem that many inventors have endeavoured to solve is that of combining in a simple apparatus both gas engine and pump. No practical solution of this problem was found prior to the inventions made by Mr. H. A. Humphrey, M.Inst.C.E., in 1906 and the following years. Previously every inventor had provided that the explosion should drive the water to be pumped past a non-return valve or valves, so that water could not return; this was natural, as these are the lines on which all ordinary pumps are constructed. Again, too little attention was given to the question of compressing the combustible mixture before ignition, although the increased efficiency which such compression involved was fully recognised in connexion with ordinary gas engines. Explosions give rise to very rapid increases of pressure and apparatus in which a heavy liquid like water has suddenly to open valves and be driven through valve ports under violent impulses is necessarily liable to trouble and disaster. It was therefore not surprising

that no internal-combustion pump had proved successful when Mr. Humphrey took up the problem and introduced an entirely new principle into engineering which promises to revolutionise the present methods of pumping water.

The genesis of Mr. Humphrey's ideas is interesting; they were largely the consequence of the failure to give the gas turbine practical shape. Mr. Humphrey had spent much time on the gas-turbine problem and the apparently hopeless results led him to devise some other means of converting gas power into mechanical power. He came to the conclusion that the energy of a combustible mixture might best be used to put water under pressure or to raise it to a height: hence most of his numerous patents bear the title "Improvements in Methods of raising or forcing Liquid and Apparatus therefor." An examination of his patents leads one to conclude that as regards broad principles he has covered the whole range of application. The claims run into hundreds: in an article of this kind it would be out of place even to quote a few of them; instead, we shall endeavour to explain the more general features.

Briefly stated, Mr. Humphrey's invention consists in utilising the energy of expansion of an ignited combustible mixture to cause the free reciprocation of a column of liquid in a play-pipe, the momentum of the moving column being utilised to carry through all the functions of a complete cycle. In many cases the explosion takes place in direct contact with one end of the liquid column but this is not a necessary condition, though the absence of solid pistons makes the simplest arrangement.

In the first Humphrey Pump which we shall describe, which is suitable for a direct lift without suction, the water column, which also acts as both piston and fly-wheel, has four unequal strokes such as theory requires when expansion is carried to atmospheric pressure. These strokes are: a long stroke during combustion and expansion, another long stroke during exhaustion, a shorter stroke during suction and a still shorter stroke during compression. There is no valve across the play-pipe (sometimes called the discharge pipe) at any point, so that the water has a free passage from the explosion chamber to the water tower, whence the water is led away at the higher level.

The action of such a pump can be best explained by reference to fig. 1. The pump proper is built up from three main castings:—C is the combustion chamber connected by means

of the bend B to the water valve chamber W, the discharge pipe D and thus to the water tower WT; the suction tank ST embraces the valve box chamber, as shown, so that there is a free access of water to all the water valves V. These water valves are plain mushroom valves opening inwards and held on their seats by light springs. In the top of the combustion chamber are fitted an inlet valve A and an exhaust valve E. A simple interlocking gear is arranged between these two valves, so that when valve A opens and closes it locks itself shut and releases the valve E; when valve E opens and closes it locks itself shut and releases valve A. Consequently each time that suction occurs in the chamber these valves open in turn.

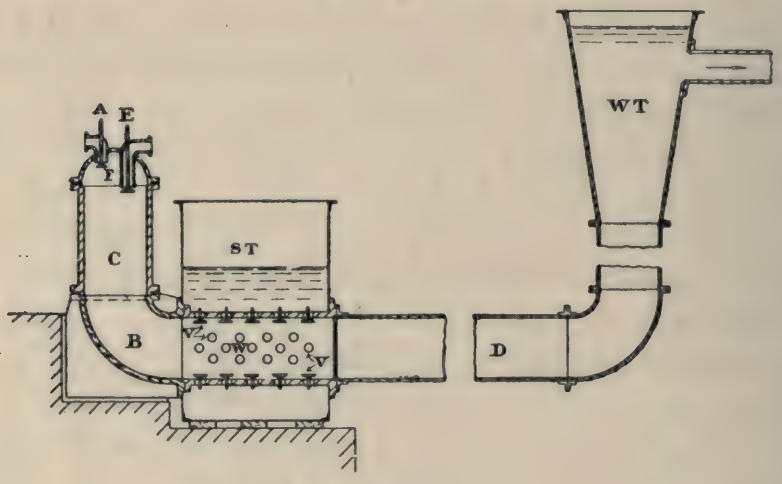


FIG. 1.

Imagine a charge of gas and air to be compressed in the top of chamber C and fired by a sparking plug which projects through the top casting. All the valves are shut when explosion occurs, so that the increase in pressure drives the water downwards in the pump and sets the whole column of water in the discharge pipe in motion. The column of water attains kinetic energy while work is being done upon it by the expanding gases, so that when these gases reach atmospheric pressure the column of water may be moving at, say, six feet per second. The motion of this column of water cannot be suddenly arrested; hence the pressure in C tends to fall below that of the atmosphere and the exhaust valve E opens and also the water valves V. Water rushes in through the water valves mostly to follow the moving column in pipe D but partly to rise in chamber C in an effort to reach the level inside the chamber

which exists in the suction tank. When the kinetic energy of the moving column has been expended in forcing water into the water tower, it comes to rest and as there is nothing to prevent a return flow, the column starts to move back towards the pump, gaining velocity until the water reaches the level of the exhaust valve, which it shuts by impact. A certain quantity of burnt products is now imprisoned in the cushion space F and the energy of the moving column is expended in compressing this gas cushion to a greater pressure than that due to the static head of the water in water tower W T. Hence a second outward movement of the column is produced and when the water reaches the level of valve E the pressure in the space F is again atmospheric; further movement of the water opens valve A against a light spring and draws in a fresh charge of gas and air. If there were no friction, the water would fall to the same level as that from which the last upward motion started but the amount of combustible charge drawn in is slightly less than this movement would represent. Once more the column of water returns under the pressure due to its elevation and compresses the charge of gas and air, which is then ignited to start a fresh cycle of operations.

The ignition is timed by a small apparatus somewhat resembling an ordinary engine indicator which closes the electric ignition circuit at the point of maximum compression; it is effected by an ordinary small battery, trembler coil and sparking plug such as are used in motor-car work.

It should be noticed that the only moving parts, namely, the valves, open and shut under slight changes in pressure and are all on their seats when explosion occurs; consequently, there is no shock and the pump works quietly and smoothly.

In starting the pump for the first time, compressed air is allowed to flow into the combustion chamber until the volume of air introduced is rather larger than the usual charge. The exhaust valve is then suddenly opened by means of a hand lever and the escape of the compressed air permits a movement of the water column which gives the cushion and suction strokes and so draws in a fresh combustible charge, which, on ignition, starts the pump working regularly. If the pump is stopped when in regular work, it always stops with a fresh charge of gas and air in the combustion chamber; the restarting of the pump is simply effected by switching on the

ignition current. The pump can therefore be started and stopped from a switchboard.

A pump of the type just described was tested by Prof. W. C. Unwin, F.R.S., M.I.C.E., in September 1909; using a pump of only 16 h.p. delivering 1,621 gallons per minute to a height of 32·87 ft. and making 13·8 complete cycles per minute, the fuel consumption was 1·063 lb. of anthracite per water-horse-power-hour.

In the official report that was issued, Prof. Unwin states that "the fuel consumption in these trials, reckoning on the work done in lifting water, was less than any other 'pumping arrangement,' either by gas or steam, hitherto recorded."

Prof. Unwin adds that "the pump works absolutely automatically, without attention when once adjusted and starts with a charge of compressed air with the greatest ease and certainty. Starting all cold, an explosion occurred in five seconds and the pump continued working regularly. In these trials the ignition was effected by a small motor-car battery and small 'Reliance' motor-car igniting plug. The pump worked throughout the trials with varying lift without any change in the gas and air mixture or any alteration in the cushion volume. Also without a single misfire or hitch of any description."

Later tests carried out on a pump of twice the size of that tested by Prof. Unwin have afforded the following results:

Gallons of water raised per minute	3,150
Actual lift of water	35·6 ft.
Horse-power in water lifted	34
Cycles per minute	12·2
Cubic feet of 150 B. Th. U. gas per horse-power-hour	73
B. Th. U. per water-horse-power-hour	10,950
Thermal efficiency (on water lifted)	23·1 per cent.
Equivalent anthracite coal per water-horse-power-hour	0·95 lb.

Before leaving this type of pump it is interesting to note that the Metropolitan Water Board, as the result of a most thorough investigation by a special committee, have decided to adopt Humphrey pumps for their pumping installation at Chingford. The quantity of water to be pumped is 180,000,000 gallons per twenty-four hours and the lift 25 to 30 ft. For this duty there will be erected five Humphrey pumps, four of which will each have a capacity of 40,000,000 and one of 20,000,000 gallons per day; thus each of the larger units will have an output equivalent

to the daily average of the water supply of Manchester and Salford. The Committee's Report for these pumps showed a saving in first cost of £19,000 on the complete installation as compared with triple-expansion steam engines running at 350 revolutions per minute and coupled to centrifugal pumps. Moreover, the fuel consumption of the Humphrey pumps is only about half that of the steam plant.

At the Brussels Exhibition of 1910 a Humphrey pump was shown in operation and was awarded two "Grand Prix," one in the class of gas engines and one in the class for pumps. The pump in question was delivering water at the rate of 6,000,000 gallons per day.

At the Turin Exhibition of the present year a Humphrey pump delivering at the rate of 16,000,000 gallons per day is to be seen in operation. It was constructed by the Tecnomasio Italiano Brown Boveri, who are the sole licensees for Humphrey pumps and compressors in Italy.

Before proceeding to describe other types of Humphrey pumps, it may be well to state shortly some of the general considerations which render Mr. Humphrey's invention of public interest, particularly to engineers the world over.

We live in an age of power-driven machinery. The power is derived from heat and for all practical purposes the heat is derived from our coal supplies, as in this country the other sources of energy are comparatively insignificant. Our coal mines are not inexhaustible; indeed, the President of the British Association, following the example many have set, has just recently reminded us of the alarming rapidity with which we are depleting our store and has thus emphasised the urgency of conserving our greatest material asset. We must be less prodigal in our use of coal. Economy is every day more important, more pressing, so that those who produce a machine wherein the heat latent in a pound of coal will give more external work than has hitherto been obtained may well be considered public benefactors. The reciprocating steam engine has been and is a good servant but we need a better and continuous efforts are being made to find it.

Two outstanding departures from the beaten track of the reciprocating steam engine development have been mentioned, namely, the gas engine and the steam turbine. The former at first sight might seem the more revolutionary innovation,

since, for the first time, combustion of the fuel was made to take place within the engine itself, whilst the steam turbine retains the boiler as an essential auxiliary. But from another point of view the steam turbine, with its absence of all reciprocating parts, such as piston, connecting rod and crank shaft, bears less likeness to its progenitor than does the gas or oil engine. Both were great and successful efforts to cast off the trammels of "previous practice" and to revert to first principles for a fresh start. Each of these two prime movers is ousting the reciprocating steam engine from an increasing portion of its domain and has added its quota to the solution of the great problem.

In the invention which promises to rival and finally to surpass in importance the gas engine and steam turbine at least in many fields of usefulness, Mr. Humphrey casts back to first principles and starts out in a new direction. The prime mover that can raise water or put it under pressure can be so arranged that it will readily give up its energy in other forms. The Humphrey pump combines the novel features of both gas engine and turbine, in that, whilst the combustion of the fuel takes place within the pump itself, it has also the advantage that there are no reciprocating parts with the exception of the water to be pumped and this swinging freely controls the cycle of operation.

The absence of solid pistons, fly-wheels, gearing bearings, connecting rods and cranks, together with cooling jackets, cams, shafts, etc., is an advantage of the Humphrey system which all can appreciate; the extent to which simplicity has been carried can best be realised when it is known that a pump can be made only with *four* moving parts, namely, the two valves—one for admission and one for exhaust—one water valve and one small paddle moved by the liquid to effect ignition.

So far we have, we hope, made quite clear the principle involved by reference to the simplest type of apparatus. The importance of the invention has been indicated and a few developments may now be considered.

We will now describe the double-barrelled pump which is diagrammatically illustrated in fig. 2, in which there are two combustion chambers A and B in which explosion occurs alternately. In each chamber the usual cycle is followed of compression, expansion, exhaustion and the taking in of the new charge,

thus again involving four strokes of unequal length. In this case the water column has only one outward and one inward movement per cycle but the expression "two-stroke" would here be misleading, unless one remembers that it applies only to the water column and does not refer to what occurs in the combustion chambers.

Let us follow through the cycle in this case, assuming that a compressed combustible charge exists in the top of chamber A and that B is full of water. Explosion and expansion in A cause the outward propulsion of the water column and when atmospheric pressure is reached the exhaust valve of A opens, water flows in to follow the column and to rise in A expelling burnt products; as there is insufficient pressure to maintain the water in B, the level therein then falls, causing an intake into B

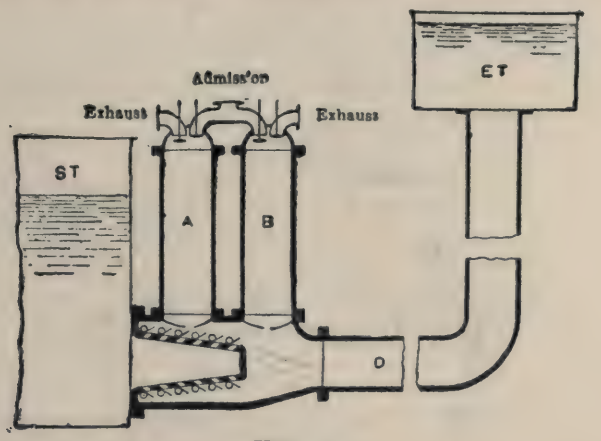


FIG. 2.

of a fresh combustible charge. All this takes place during the outward movement of the column of water. Then the column which is coming to rest commences to return towards the chambers; it finds that the remaining burnt products in A can be readily sent out through the still open exhaust valve, whereas the new charge in B cannot escape, as the admission valve which admitted the charge is now shut under the action of its spring; consequently the column rises at first in A until the exhaust valve is shut and then in B, compressing the new charge. Ignition in B starts a fresh cycle with the functions of A and B reversed.

There are important differences to be noted between the action of this pump and of the single-barrel pump first described. To begin with, the new charge is taken in merely by water falling under the action of gravity and does not

depend upon the compression and expansion of an elastic cushion. The clearance spaces at the tops of the combustion chambers may therefore be reduced to the very small volume needed to bring easily to rest that portion only of the column which rises in the chamber, the main body of the column being brought to rest by the compression of the new charge. Again, for a given height of lift the compression pressure attained is higher in the double-barrel pump, because the returning column gains speed while exhausting the last part of the burnt products and has thus obtained a store of kinetic energy before starting the compression stroke proper.

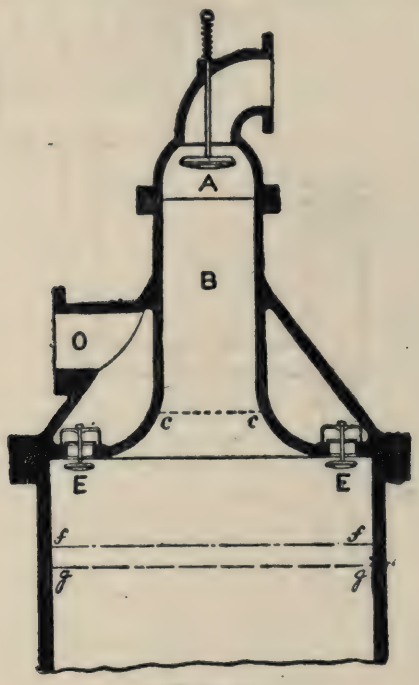


FIG. 3.

A two-cycle pump has been invented by Mr. Humphrey in which there is only one combustion chamber. The diagram of the top of this pump is shown in fig. 3, where A is the admission valve at the top of a tall but narrow part of the chamber B in which the full charge volume extends down to the level of *c c*. A number of exhaust valves E lead to a common exhaust outlet O, which may be fitted with a non-return valve or each exhaust valve may carry a light non-return valve on its spindle as shown. The level at which expansion reaches atmospheric pressure is, say, *f f*; this level having been reached by the water, its further movement may either draw in pure air followed by combustible mixture

through the valve A or such scavenging air and combustible mixture may with advantage be supplied under sufficient pressure so that some of the burnt products may be positively scavenged out from the combustion chamber; the remaining products may be swept out when the liquid returns under the head or pressure to which it has been raised and rising from the level *gg* reaches the exhaust valves E, shuts them and by the virtue of its momentum continues its movement upwards and compresses the combustible charge contained in B ready for a fresh ignition to start a new cycle. We have thus only one out-stroke and one in-stroke of the liquid column for each cycle of operations.

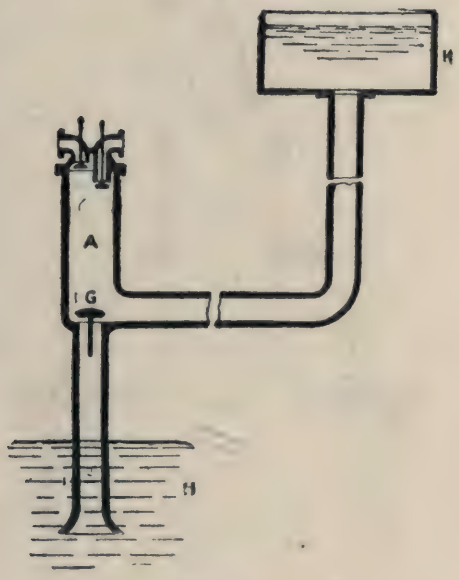


FIG. 4.

We will now take the simplest case of a pump having a suction lift, as illustrated in fig. 4, in which the cycle is as follows:

1. Explosion of compressed combustible charge in A, driving water to high level tank K, continuation of expansion stroke below atmospheric pressure owing to the existence of non-return valve in exhaust pipe; consequent lowering of pressure till water is raised from low level tank H, past valve G, until water comes to rest and valve G shuts.

2. Return movement of water column towards chamber A, giving exhaust and cushion stroke.

3. Cushion expansion and intake of a measured combustible charge into A and expansion of this charge below atmospheric pressure.

4. Second return of water column towards A, giving compression stroke.

It is obvious that a partial vacuum corresponding with the height of the suction lift must be produced in the chamber A in order that the fresh liquid may be drawn in past the valve G; this necessitates the size of the combustion chamber A being made large enough to admit the necessary expansion down to the partial vacuum required. For large suction lifts this would prove a disadvantage and it is better in such

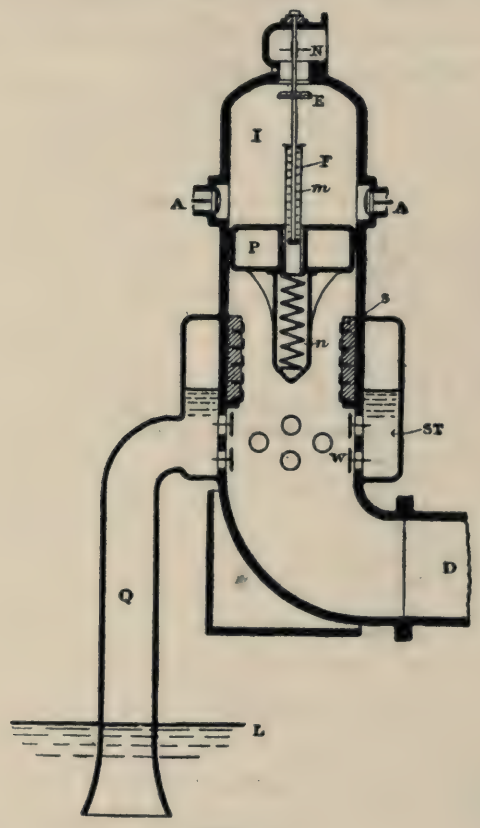


FIG. 5.

cases to make the vacuum which draws in fresh liquid independent of the degree of expansion of the combustion gases. Such an arrangement is shown diagrammatically in fig. 5, where I is the combustion chamber and P is a piston sliding easily in the chamber the motion of which can be arrested by the elastic buffer-stop S, here shown as a number of rubber ring but which can be an air-cushion or any suitable form of dash-pot. Pipe Q conveys the low level liquid to the closed suction tank S T which surrounds the water valves W and has an air cushion on top. As the piston P

rises, the exhaust valve E is shut by the sleeve F and a combustible charge compressed in the top of the chamber I. This charge is fired, giving the working stroke; the piston P in being driven downwards first opens the exhaust valve E by means of the sleeve F, then uncovers the admission valves A for fresh combustible charge and is brought to rest on the spring stop S. At this moment the liquid column in pipe D has considerable velocity and the motion of the piston P having stopped, fresh water is drawn in through the water inlet valves W to follow the outwardly moving column in the pipe D. After the admission valve ports are uncovered by the piston, its further movement draws in a fresh combustible charge through valve A and this charge lies at the bottom next the piston with the hot exhaust gases above. When the return movement of the column in pipe D takes place, valves W close and the piston P is forced upwards by the liquid, so driving out the exhaust products through valve E until this valve is closed and the compression of the fresh charge remaining in the chamber I is completed. The springs *m* and *n* enable the valve E to be operated without shock. It will be noted that when the exhaust valve E is forcibly opened a shorter stroke and higher mean pressure can be obtained than when expansion is carried to atmospheric pressure. The pump is here shown of the two-cycle type but a similar arrangement can be applied for the four-stroke cycle type.

All the types of Humphrey pumps so far described have their lift limited to approximately 40 ft., since this represents about the mean effective pressure of the ignited expanded gases during the long outward stroke; to effect lifts higher than 40 ft. Mr. Humphrey has invented what he calls an "intensifier." This is practically an air cushion hydraulic ram by the employment of which the lift may be increased to almost any desired extent and is rendered independent of the working pressures in the explosion cylinder. The idea is first to allow the water column to gain velocity and then to utilise its kinetic energy (*a*) to compress an elastic fluid, (*b*) to deliver water under the pressure to which the elastic fluid has been compressed. Fig. 6 will serve to make the cycle clear.

A and B are the barrels of a two-barrel pump and at the end of the play pipe D there are two air vessels E and F, the latter being large enough to give a continuous flow at

outlet O and to maintain a practically uniform pressure. The smaller air vessel E is fitted with a downwardly projecting pipe K, open to the atmosphere at the top and carrying a valve L at its lower extremity arranged so that it closes under the action of rising water. The cycle starts with explosion, all valves except L being shut and the water level as shown. While the water level in E is rising to L, air is merely being discharged into the atmosphere and as no work is being done by the column of water it gains speed until valve L is shut by impact. Imprisoned in E there is now a definite quantity of air, which suffers compression until its pressure reaches that at which the high pressure water valves W can open and allow the remaining kinetic energy of the column to force water into F. Valves W close when the column comes

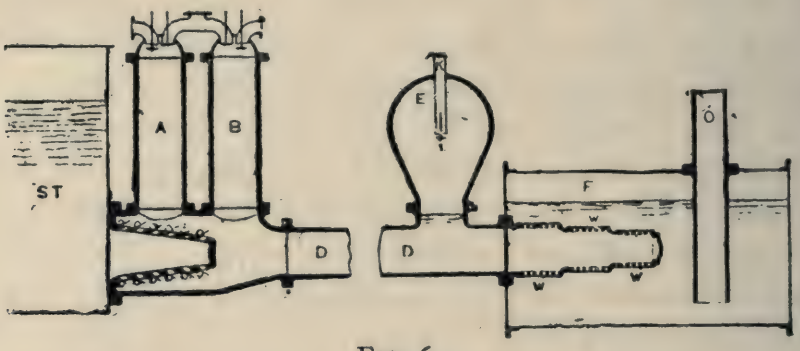


FIG. 6.

to rest but there remains enough energy in the compressed air in E to give, by expansion, the return flow, which causes exhaustion in A and compression of the fresh charge in B to start a fresh cycle. When the water level falls below valve L, this valve opens and air is admitted into E for the rest of the return stroke.

Now it is easy to see that if the pipe K is made vertically adjustable with regard to E, the point of the cycle at which L shuts can be varied and more or less air entrapped in E at will. But the amount of energy stored in this air will also vary with its quantity, for we assume that the degree of compression remains constant and is indeed fixed by the pressure maintained in F. Consequently the ratio of the total energy of the working stroke to the energy stored in the compressed air in E can be made anything desired; in other words we can obtain any compression pressure of the new charge in B which we like and this independently

of the water lift. The advantage is obvious, for compression pressures equal to those in modern gas engines can be employed with a corresponding increase in thermal efficiency. Further, by manipulating the position of pipe K, a given pump can be made to meet any conditions as to height of lift, for if the lift increases K can be raised so that the energy stored in the air in E remains the same, there being now less air but at a higher pressure.

At this point we will indicate the method by which the Humphrey system can be applied to power production. We have seen that a simple modification of the pump first described enables water to be put under a pressure comparable to that which established practice regards as the best for use in a water turbine or impulse wheel. The water is then passed through an ordinary water turbine and is led back to the pump to be used over and over again. Here then is a power unit which in many respects is ideal. Pump and water turbine work at temperatures which avoid the necessity for special water-cooling devices such as are necessary in the case of gas engines; and the advantages of the water turbine, such as continuous rotation, are utilised.

The lubrication problem is reduced to such a point as to be almost abolished.

The plant can be instantly started up from "all cold" and stand-by losses are therefore eliminated. The surfaces subject to wear and corrosion are so slight that the depreciation of the whole plant is very small. The combination can be so designed also that it will be in large measure "fool proof" and can be served by unskilled labour to an unusual extent. Above all, even in the present experimental plant, the fuel consumption is remarkably low and will undoubtedly be still further reduced in the future.

A short digression may now be made to describe an important development of the arrangement shown in fig. 6, which shows an apparatus wherein, at each cycle, air is drawn into and rejected from the vessel E. Let us suppose K is connected to a supply of combustible mixture instead of opening into the atmosphere, we shall then have an automatic pump for taking in a mixture and discharging it under pressure. If the discharge is into a reservoir from which combustion chambers A and B can be supplied, we have at once a

means of quickening the cycles and greatly increasing the output of an apparatus of a given size. It is convenient to substitute for vessel E two vessels, one for air and one for gas, so as to maintain the combustible constituents separate until they enter the combustion chambers. If the first portion of the out-stroke of the water column is allowed to reject the surplus air and gas back to the sources of supply, then the action throughout the cycle is precisely that described when using the single vessel E, except that a larger proportion of the total energy is absorbed in the compression of air and gas but the excess is given out again during the expansion of the pre-compressed charge in either A or B. The chief advantage arises from the more rapid working, as there is no longer any need to wait for the water level in A or B to fall under the action of gravity when the charge is being taken in. In fact the apparatus becomes practically independent of the water level on the supply side. The 1,000 h.p. Humphrey pump now under construction in Germany is designed to operate with pre-compression of the charge, the result being that the dimensions are very moderate and the pump itself occupies no more space than a 1,000 h.p. tandem gas engine.

Sufficient has been said to show that the variety of types of the Humphrey pump may be multiplied indefinitely whilst still maintaining the same principles of operation; it may be mentioned also that pumps have been designed in which, instead of a sudden explosion, the combustible mixture is injected and burnt at approximately constant pressure. Humphrey pumps have been successfully operated on producer gas, coal gas, water gas, petrol and paraffin and patents have been taken out for designs specially suitable for use with crude oil as fuel; but space does not permit of these modifications being described, more especially as this article will be concluded by a description of the Humphrey Air Compressor.

If the column of water oscillating in the play pipe of a Humphrey pump is used as a water piston and caused to rise and fall in an air vessel fitted with suitable valves for the inlet and outlet of air, the combination constitutes an air compressor of a very efficient type and promising many advantages. Let us take the case of a single-barrel pump and a single air vessel, as illustrated in fig. 7. A is the ordinary pump chamber and C the air compressor chamber; we may call these for con-

venience the "pump" and the "compressor" respectively. The inlet valve *i* for gas and air and the exhaust valve *e*, for burnt products exist as before; but the exhaust pipe and valve are adjustable vertically so as to vary the cushion space in the top of A. There is another pipe *p* fitted with a valve *r* and also adjustable vertically. A reservoir X, such as an old boiler shell, is connected by a flexible pipe to *p*. Corresponding parts are fitted to the compressor; thus *f* is an air inlet valve, *g* an outlet valve for air which is shut by the water and *h* a non-return valve for the compressed air. The dip pipe *q*, with its valve *s*, gives communication between the atmosphere and the compressor. The outlet pipe and the dip pipe are vertically adjustable as in the pump. Lastly, suppose the play pipe D filled with water,

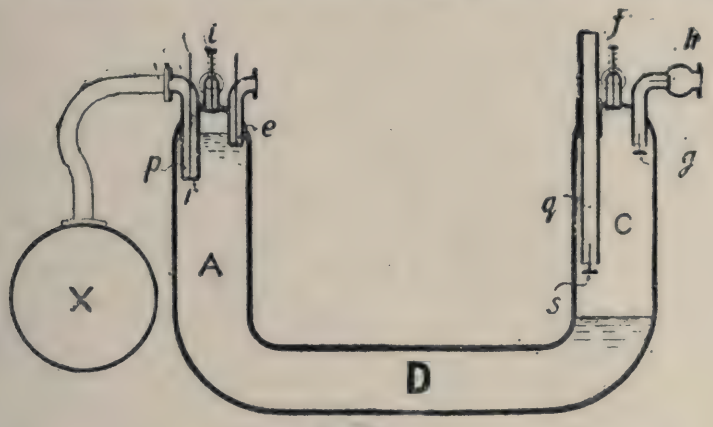


FIG. 7.

then we have all the elements required for working under the most varied conditions.

The cycle can be explained by assuming that, as shown in the figure, the water level is well up in A and low in C, and that a compressed charge is ignited in A. Expansion occurs, driving down the water in A and up in C. At first air escapes from C through valve *s* but when the water reaches this valve it is shut and the remaining air is trapped and suffers compression until the pressure of discharge is reached, when *h* opens and compressed air is delivered. Next the water reaches valve *g* and shuts it so that no more air can escape; the water column is then brought to rest by the continued compression of the remaining air, which forms a cushion. Meanwhile in chamber A expansion to atmospheric pressure allows the exhaust valve *e* to open; the continued stroke then draws in scavenging air (the scavenging valve being omitted from the figure for the sake of simplicity) but

valves *i* and *r* remain locked. Now the column begins to return, owing to the expansion of the compressed cushion in C and this causes exhaust and cushion in A; at the same time there is a fall of pressure in C to atmosphere, then valve *f* is opened against a light spring and a fresh supply of air enters.

So far we have had one outward and one return stroke but the expansion of the cushion in A starts a second out-stroke which results in drawing in a new combustible charge into A through valves *i* and *r*, which were released on the shutting of the exhaust valve *e*. As the second out-stroke is nearly as long as the first, it follows that too much combustible mixture is drawn in; the surplus is got rid of on the second return stroke by allowing it to escape into the receiver X and to raise the pressure slightly therein. Valve *i* was shut by its spring but *r* waits to be shut by the action of the water; when this occurs chamber A contains a definite volume of combustible mixture, the size of charge being fixed by the height of *r*. Finally, we have only to observe that the second out-stroke towards C merely compressed the air in C without causing delivery to take place but storing in the air the energy given out by the expansion of the cushion in A. The air in C now expanding gives the compression stroke in A and the combustible charge is ready to be ignited to start the next cycle.

Perhaps the whole cycle, which may take two seconds to accomplish, sounds a little complicated but if so it is merely because of the length of the description. It may be easier to grasp the action if it is set out to show the simultaneous steps in A and C, thus:

	A	C
1st Out-stroke .	Expansion to atmosphere. Intake of scavenging air.	Expulsion of air till water shuts valve *s*. Compression and discharge of compressed air till water shuts valve *g*. Cushion till water comes to rest.
1st In-stroke .	Exhaust till water shuts valve *e*. Cushion till water comes to rest.	Expansion of cushion to atmosphere. Intake of fresh air.
2nd Out-stroke.	Expansion of cushion to atmosphere. Intake of combustible charge in excess.	Compression of air but not sufficient for further delivery.
2nd In-stroke .	Rejection of surplus charge till water shuts valve *r*. Compression of charge till water comes to rest.	Expansion of compressed air.

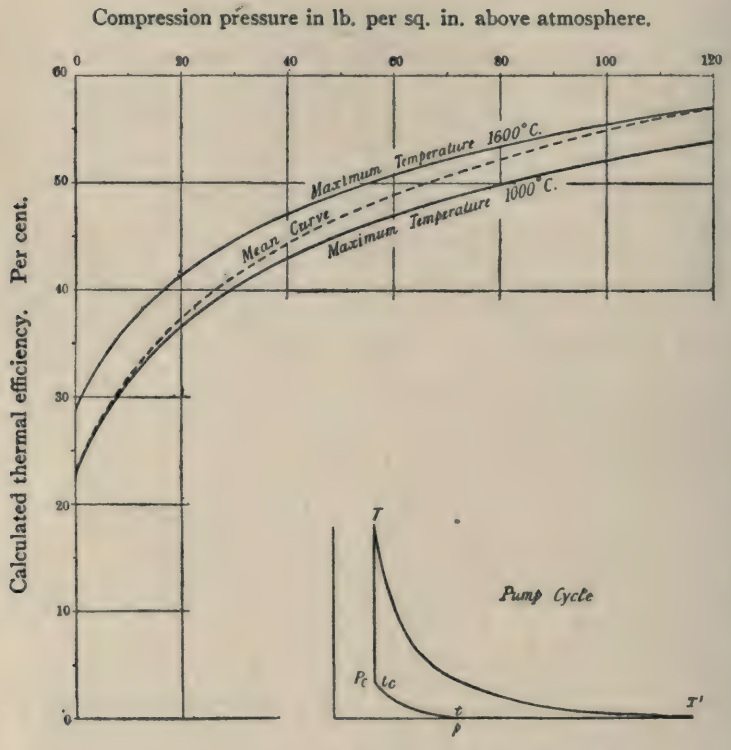
It will be evident that in the next cycle, when admission of combustible charge occurs, the surplus last rejected into reservoir x will want to enter first, because it is under slight pressure, whereas inlet valve *i* has to be sucked open against its spring. Consequently x will empty to atmospheric pressure each time and only the additional mixture required will be taken in through valve *i*. A slight modification may be made by transferring valve *i* from the pump to the reservoir x.

The flexibility of the air compressor can now be studied. To begin with the pump side, the level of the inlet valve *e* and the rejected charge valve *r* are assumed to be variable, although fig. 7, being merely a diagram, does not show how the pipes carrying these valves are moved vertically. As the level of these two valves controls the amount of charge ignited at each cycle and the amount of the cushion space, their regulation is all that is required to increase or diminish the energy developed per working stroke. On the compressor side, the position of the valves *g* and *s* controls the cycle of operations on this side of the apparatus and renders it possible to compress either a large volume of air to a low pressure or a smaller volume of air to a high pressure or to make any intermediate changes which may be desired.

Thus all the conditions of output, up to the full limit of the compressor, may be governed at will; and for all ranges the compression pressure of the new charge may be kept up to the required degree, so that the apparatus works at its maximum efficiency throughout the whole range. The amount of water which oscillates between the chambers should theoretically be altered along with the total capacity per working cycle; but the reason for this is merely to prevent the last portion of each down-stroke from being wasted by taking in surplus combustible mixture into one chamber or surplus air into the other chamber to an undue extent. If the surplus of the combustible mixture is unnecessarily large, the extra amount rejected will increase the pressure in the reservoir x and this increase of pressure may be made to govern automatically a water supply and so bring up the total volume of reciprocating water or to allow part of the water already in the apparatus to escape, so as just to keep a small amount of excess charge for each cycle no matter what may be the output of the pump.

Turning now to the question of thermal efficiency, the curves

shown in figs. 8, 9 and 10 are taken from the paper read by Mr. Humphrey before the Institution of Mechanical Engineers and show the relative efficiency as between the Humphrey pump cycle and the ordinary Otto cycle calculated by means of the usual formula and upon the assumptions stated on the diagrams.



Formulæ for pump-cycle diagram.

$$\text{Efficiency} = 1 - y\frac{T^1 - t}{T - t^c}$$

$$T^1 = t\left(\frac{T}{t_c}\right)^{\frac{1}{y}}$$

$$t_c = t\left(\frac{p_c}{p}\right)^{\frac{y-1}{y}}$$

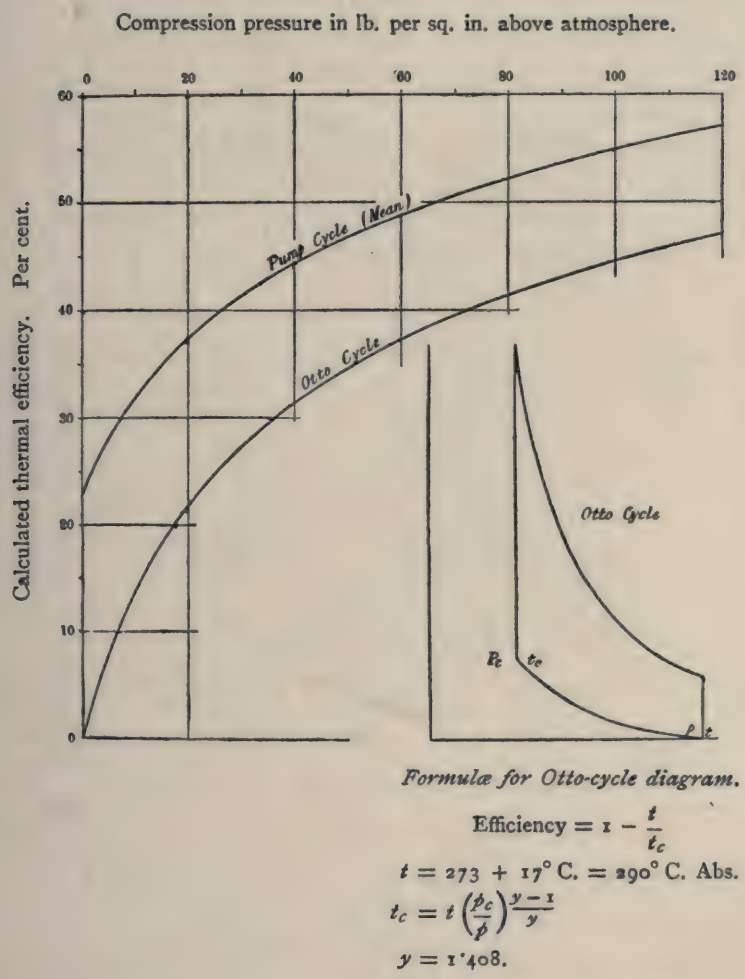
T = (for curve at 1,000° C.) = 273 + 1,000° C. = 1,273° C. Abs.
T = (for curve at 1,600° C.) = 273 + 1,600° C. = 1,873° C. Abs.
y = 1·408.
t = suction temperature = 17° C. = 290° C. Abs.
Adiabatic compression and expansion.

FIG. 8.—Theoretical efficiency curves, pump cycle, air standard.

These calculations are based on the assumption of the constant specific heat; but Prof. Dr. Eugen Meyer, of Charlottenburg, has made the very laborious calculations involved when taking the true specific heat of each constituent of the gas mixture and allowing for the variations of the temperature involved at each point on the curve. The absolute results are affected but the

relative results remain approximately the same and for present purposes it is sufficient to take the curves as here given.

It may be mentioned, however, that with a compression pressure of eleven atmospheres absolute the theoretical thermal efficiency of the Humphrey cycle is 52·5 per cent., whereas that of the Otto cycle is only 40 per cent. when all corrections for



Formulæ for Otto-cycle diagram.

$$\text{Efficiency} = 1 - \frac{t}{t_c}$$

$t = 273 + 17°$ C. $= 290°$ C. Abs.

$$t_c = t\left(\frac{p_c}{p}\right)^{\frac{y-1}{y}}$$

$y = 1{\cdot}408.$

FIG. 9.—Theoretical thermal efficiency curves—air standard.

varying specific heats are allowed for. Such compression pressures have not yet been adopted by Mr. Humphrey but are to be tried in the 1,000 h.p. German pump already referred to. In the meantime, when using only very moderate compression pressures of under 50 lb. per square inch, an actual thermal efficiency of over 23 per cent. has been obtained on a four-cycle Humphrey pump. This corresponds to 0·95 lb. of anthracite coal per water-h.p.-hour and was obtained on a lift of only

35 ft. It may be expected therefore that as the compression pressure increases the efficiency will also increase; there is every reason to believe that when the compression pressure reaches that employed in ordinary gas engines, the best

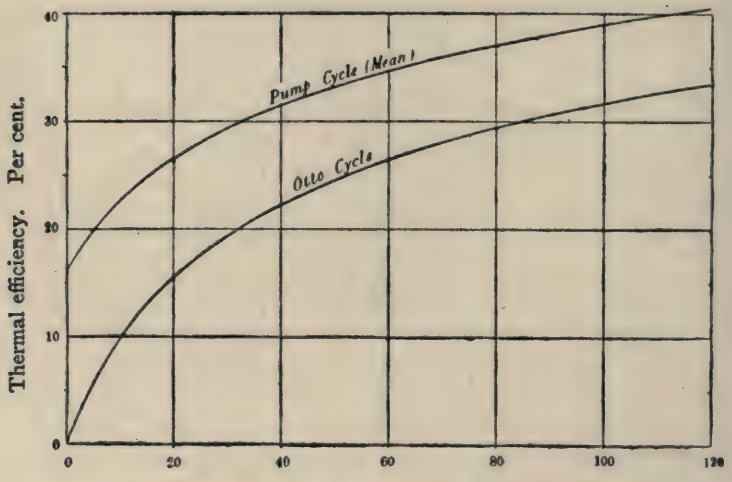


FIG. 10.—Practical efficiency curves: 0·71 of air standard.

efficiencies attained in gas engines will be reached and probably surpassed by the Humphrey pump.

Readers who desire to study this subject in greater detail may be referred to Mr. Humphrey's papers, especially to that read before the Manchester Association of Engineers last year.

THE ETHICS OF FOOD

III. BREAD

(Second Article)

Since the previous article was written (Science Progress, April 1911) two important reports dealing with Bread and Flour have been issued by the Local Government Board, both the work of Dr. J. M. Hamill.[1] The subject was also brought under discussion at the recent meeting of the British Association at Portsmouth. As the statements in Dr. Hamill's reports are likely to be regarded by the public as authoritative and may even form a basis for legislation, it appears desirable to extend the scope of the previous article and to examine them critically at some length.

It will not be denied that in a Government Report on such a subject the scientific evidence available should be marshalled without bias and that the conclusions drawn should be only those that are justly deducible from the facts. In his earlier report, Dr. Hamill appears to have erred in allowing the popular prejudice of the moment to affect his judgment; his later report is more satisfactory in this respect and may be recommended as a reasoned and temperate document.

It is impossible to survey the situation created by the recent newspaper campaign without realising the difficulties with which we are faced. Obviously it has been for the most part in the hands of irresponsible literary agents whose chief desire has been to make copy and secure advertisement; with few exceptions, those who have been *bona-fide* contributors to the discussion have been actuated by sentiment alone: it cannot have been otherwise, as the knowledge is wanting which would

[1] "Reports to the Local Government Board on the bleaching of flour and the addition of so-called 'improvers' to flour." By Dr. J. M. Hamill and Dr. G. W. Monier-Williams. *Food Reports*, No. 12 (Cd. 5613), Price 3*d*.—"Report on the nutritive value of bread made from different varieties of wheat flour." By Dr. J. M. Hamill. *Food Reports*, No. 14 (Cd. 5831), London: Wyman & Sons, Price 3*d*.

enable us to arrive at valid conclusions on most of the issues raised. But in the background we may discern a public feeling, hopelessly irrational in character, against any scientific interference with our food supply. In spite of all that is daily said and written as to the need of introducing scientific methods into manufacturing industries and daily life, in spite of the vaunted modernisation of our habits, we are still dominated by the conservatism which is fortunately innate in us. The introduction of new methods, new appliances, new customs, however advantageous they may be, is resisted as long as possible and the national press harks back longingly to the good old times: the bread of to-day, we read, is inferior to that of our youth and we are urged to return to the old practices in milling and baking.

The objection that has been taken to bleaching flour would seem to be quite illogical in view of the public demand for sugar coloured blue so as to counteract the yellowness which is characteristic of all but the most highly refined material. Colour indeed is an ethical factor of no slight importance in connexion with food; thus milk and especially butter are demanded dyed yellow as proof of quality, though every one is aware that both articles are naturally of a pale hue. Other foods are dyed red before they are placed on the market.

An even better example of prejudice was that afforded, some time ago, in the case of the water supply of one of our northern cities. Being exceptionally pure, this water was found to attack the lead service pipes so that it became poisonous. It was proposed to overcome the difficulty by the well-known remedy of adding a little lime to harden the water. Certain objectors to the proposal thereupon started a press campaign, of the usual sensational character, with the popular cry that it was intended to poison the water by the addition of deleterious chemicals and so great was the public outcry that the scheme had to be abandoned. Apparently those who protested were quite unaware of the fact that London and all towns situated in limestone districts habitually consume water containing naturally a large proportion of the "chemicals" which it was proposed to add to the water in question.

Even more illogical is the attitude towards the addition of preservatives which are almost universally admitted to be harmful and as such are forbidden by the very strictest legislation in many foreign countries: in this country, the addition of boric

acid to all sorts of food materials is openly tolerated, indeed it is difficult, if not impossible, at the present time, in many important markets, to purchase butter which is free from this preservative.

All evils of the flesh have been attributed of late to White Bread. The outcry might be safely ignored had not many taken part in it who might have been expected to know better and to have been guided by facts rather than by fancies. The Local Government Board is therefore to be congratulated on having instituted an inquiry into the whole subject and on the promptness with which their reports have been issued. It is to be hoped that at no distant date the Board will be properly organised to undertake systematic inquiry into subjects affecting the public welfare, especially into the quality of our food supplies. Our American cousins already possess such an organisation in the various Bureaus of the Department of Agriculture; these continually issue reports on the examination of foods and drugs which are of the greatest value both to the American nation and to civilisation in general.

No struggle of greater consequence has taken place anywhere during the last few years than that waged in the United States of America to secure reform in the quality of foods. Originally the position there was a very serious one, as foodstuffs of every kind were adulterated or made of inferior materials and the public imposed upon in every imaginable way without the possibility of defence. Under Dr. Wiley's leadership, a determined stand was made against the practice: when the gravity of the issue was appreciated, Pure Food Laws were readily passed by the State; and in administering these laws an enormous number of analyses have been made of every kind of food product by his Department. The results were altogether startling and innumerable prosecutions became necessary; the consequence has been, however, to render the food supply in the United States second to none in purity. Of necessity, in enforcing a principle as the only practical solution of the difficulty of deciding in cases in which experts differ irreconcilably, Dr. Wiley has been forced to err on the side of severity and regulations have been made which, in some instances, have led to an undesirable restriction of trade. Naturally the fight has been a very bitter one and the Trusts and the various vested interests concerned have sought by every means, fair and

unfair, to displace Dr. Wiley and throw scorn on his work. It is to be regretted that at times the attacks seem to have met with partial success but the writer has frequently heard it stated that Dr. Wiley has every citizen of the United States at his back and it may be hoped that nothing will happen to mar his efforts; he certainly has the well-wishes of all who are interested in securing the supply of pure food in this and other countries.

Legislation similar to that operative in America has been enacted recently in France, though of a less sweeping character; nor indeed was such necessary and this may be said to be the case in Britain also. Care must always be taken, in fact, to discriminate between practices which are either fraudulent or injurious and those for which it may be claimed that they effect improvements of natural products. Nothing is more fallacious than the idea that Nature is in any way perfect, particularly when we misapply her products. Our foodstuffs—such as wheat, for example—can scarcely be held to have been created solely for the purpose of bread-making and it is nonsensical to look upon any single material such as wheat as a perfect food for man: it is indeed surprising that this view should be so largely supported.

Bleaching of Flour

Dr. Hamill's first report deals mainly with the bleaching of flour and the addition to it of various chemicals—so-called "improvers": it will be desirable to examine it in some detail.

Bleaching of flour has been very generally practised both in this country and abroad, during several years past. No secret has been made of the practice and in the majority of cases the baker has been fully aware that he was buying bleached flour. From time to time objections have been raised to the practice; these were brought to a head in December 1908, when the United States prohibited the use of bleached flour and again when, in this country, it was sought to invalidate one of the patents relating to flour bleaching on the ground that the practice was against the public interest. Expert evidence on either side was heard at great length during the trial; eventually Mr. Justice Warrington gave judgment in favour of the process and decided that the allegation of injury had not been proved.

The reasons for bleaching are stated by its advocates to be twofold—a whiter flour is obtained and the flour conditions more quickly. On the other side it is claimed that bleaching is directly injurious to the public health, the effect on conditioning is denied and the sale of bread artificially whitened is regarded as fraudulent.

Bleaching is effected by the miller almost entirely by electrical methods. By passing an electric spark discharge through air, a small proportion of the nitrogen and oxygen are caused to combine and the bleaching agent nitrogen peroxide is produced, together with a minute quantity of ozone. The flour is briskly agitated with the electrified air during only a very few seconds; indeed, special care has to be taken to prevent the flour from being over-bleached and spoilt by excessive exposure to the bleaching agent.

The nature of the demand for a white loaf in this country has been already considered in the earlier article. As Dr. Hamill is at pains to show, English millers are forced to buy their wheat from all over the world. Some of the wheats yield flour of excellent baking qualities but which is classed as inferior in consequence of its being yellow in colour; bleaching is considered specially useful in the case of the flour from such wheats. Certain Indian varieties of wheat, Hard Plate, Walla Walla, Durum and some Russian varieties come in this category —obviously a very wide selection. Should the miller make flour from these wheats without bleaching them, he is forced to accept a lower price on account of the inferior colour of the flours though the bread-making qualities and nutritive value are in every way equal to those of the more favoured whiter flours.

The report deals very fully with the considerations influencing the amount and degree of bleaching and with the effect of bleaching on the appearance and other properties of flour; the conclusions finally arrived at are as follows:

(1) That when properly carried out bleaching has no appreciable effect on the baking qualities of flour;

(2) That bleaching cannot counteract defects in flour due to unsoundness in the wheat from which it was milled;

(3) That the improvement of flour by bleaching represents a pecuniary gain to the miller;

(4) That so long as buyers of flour attach importance to whiteness, they are liable when purchasing bleached flour to be

supplied with an article different in quality from that which they believe they are receiving.

It is a moot question what a baker expects when he buys flour largely on its colour. Obviously the loaf he makes must be a satisfactory one—large, well-risen, white and of good flavour; moreover, the number obtained from the sack of flour must be up to the average. This implies that the flour gives a proper dough and is generally satisfactory. If his anticipations are so far realised, the baker will have no cause for complaint. What is the result if the flour he has bought has been bleached? There is no evidence to prove that it will behave abnormally in doughing or fail to give the usual number of satisfactory loaves of the popular degree of whiteness; all that has happened then is that he has paid a little more for the flour on account of its whiteness than he would have paid if it had had the yellow colour proper to the wheat from which it was made. Neither the baker nor the public concern themselves in the very slightest as to the nutritive value of the loaf: if the scientist attempt to do this on their behalf, he will be unable, as the result of his chemical analysis, to find the slightest difference; nor will the physiologist, when asked to compare the relative digestive values of the two loaves, be able to distinguish between them. If such be the case, how can the buyer be said to have been defrauded? The baker could not have sold the yellow loaf at the same price as the white one and therefore would not have bought the yellow flour.

Chemical Changes produced by Bleaching Flour

The chemical changes occurring in flour as the result of bleaching are dealt with in the report. Logically the discussion should have been followed by a similar consideration of bread, since this is the form in which flour is actually used by the public. Dr. Hamill, however, is discreetly silent on this point. In properly bleached flour the chemical change produced by bleaching is exceedingly small: usually the only noticeable feature is the presence of a few parts per million of nitrites. Dr. Monier-Williams's figures, based on his own analyses of seventy samples of flour and on data afforded by the examination of 157 other English samples, show that no flour purchased commercially contained more than 4·5 parts of sodium nitrite

per million parts of flour and that at least two-thirds of all the bleached flours tested contained less than one part of nitrite per million of flour.

These figures amply serve to negative all the statements that have been made as to the introduction of large quantities of poisonous nitrites into flour during bleaching. Yet in spite of this evidence, Dr. Monier-Williams makes sweeping statements as to the injurious effect of bleaching on flour—on the basis, however, of special experiments he has made with flour containing from 100 to 300 parts of nitrite per million! He appears to have manufactured a case against bleaching.

Dr. G. W. Monier-Williams's section of the Report of the chemical changes produced by bleaching is framed on the evidence of experiments made in the Local Government Board laboratory which are described in some detail. The flour was bleached so as to contain increasing amounts up to 330 parts per million of sodium nitrite, an amount out of all proportion to the quantity present in commercially bleached flour. His conclusions are based chiefly on these over-bleached flours; indeed the experiments with commercially bleached flours recorded in his tables show almost always no action. It is somewhat difficult to understand why the deleterious changes produced by the excessive doses of nitrite should be emphasised as if the same result accompanied commercial bleaching.

In brief, the more important conclusions that he draws are (1) that the proportion of nitrites decreases considerably as the flour is kept; (2) that in highly bleached flour a considerable increase in the amount of soluble proteins and soluble carbohydrates takes place; (3) that the oil becomes oxidised and absorbs nitrogen but that no diazo-compounds are formed.

It would have been more in accordance with the experimental data which he records if it had been pointed out that commercial bleaching has no influence on flour, though gross overbleaching causes certain changes in the constituents: but even these latter appear to be of the same order as those produced during fermentation or on keeping flour, as they involve either the production of soluble proteins and soluble carbohydrates or the oxidation of fatty matter; therefore it is impossible to regard them as deleterious. Really deleterious substances such as diazo-compounds are shown not to be formed.

It is therefore impossible to regard Dr. Monier-Williams's

deductions as scientifically justified by the facts he has himself established; some explanation is required of the attitude he has adopted in coming to such illogical conclusions.

Physiological Effects

Turning to the physiological effect of bleaching, the evidence available is mainly that given by expert witnesses in the patent trial referred to. Ladd and White, experimenting in North Dakota, U.S.A., showed that rabbits were rapidly killed by alcoholic extracts of bleached flour and inferred the presence of a toxic substance, probably a diazo-compound. Subsequent observers have failed to confirm these results and they are generally discredited. Halliburton states that traces of nitrites have an inhibiting effect on the action of digestive enzymes and infers that bleached flour would act likewise; his experiments, however, are not of a character to carry any weight with those who are conversant with the behaviour of enzymes in presence of metallic salts such as the nitrites.

On the other hand, numerous investigators have failed to find the slightest toxic action associated with either properly bleached flour or its extracts. Fortunately the Local Government Board has settled this vexed question for all time by securing the assistance of Dr. A. Harden, F.R.S., a worker whose repute justifies entire confidence in his experiments. An account of his observations is published as an appendix to the report.

Dr. Harden's conclusion is very straightforward—the extract from commercially bleached flour, from overbleached flour and even that from grossly overbleached flour is apparently harmless to animals. With regard to enzymic digestion, he finds that the bleaching of flour does not in any way diminish the readiness with which it undergoes tryptic digestion in glass vessels. Peptic digestion seems to be slightly inhibited by bleaching but the differences observed were very small and it may be doubted whether they have any significance.

Flour containing ninety parts of nitrite per million was found by Dr. Monier-Williams to retard salivary digestion; commercially bleached flour had no effect. Dr. Hamill's final corollary is that highly bleached flour cannot be regarded as free from risk to the consumer and that even in the case of flour bleached by the methods in vogue at present it would

be unwise in the present state of knowledge to conclude that the process is absolutely free from risk!

There is unfortunately no word in the report as to what happens when bleached flour is made into bread but we have the authority of Snyder in America that the nitrite disappears almost entirely; indeed Jago has calculated that in order to take the maximum safe dose of nitrite, 10,000 one-pound loaves of bread would have to be eaten, which at the average rate of bread consumption would take an individual fifty-five years!

Seeing, moreover, that the yeast organism—which is certainly as delicate a feeder as ourselves and even more susceptible to the influence of foreign substances than is our own digestive apparatus—attacks bleached flour without difficulty and ferments it in a perfectly normal manner, it is open to inquire what has become of the great changes to which Dr. Monier-Williams refers. There could be no better proof that they do not exist: if they did the flour would be useless to the baker.

In our opinion, this part of the report errs in drawing conclusions not in any way in agreement with the evidence so admirably collected and discussed in its earlier pages. Disapproval of the practice of bleaching may fairly be based either on its being one that is calculated to deceive or on the ground that it is unnecessary—that is to say on ethical grounds; there is, however, not the slightest evidence that when properly carried out it has any injurious effect whatsoever.

The report is of the very greatest public value in drawing attention to the question and of even greater value in affording authoritative statements on many details which are so constantly misrepresented in the Press.

Improvers of Flour

The second part of the report concerns the addition of the so-called "improvers" to flour. Although bakers have long been in the habit of adding a variety of substances to flour when making bread, including milk, fats, starches, salts, malt extract, yeast foods, etc., it is only quite recently that millers have treated their flour with extraneous substances. No exception has been taken to any of the additions practised by bakers, excepting to that of alum, which has been proved to have a deleterious effect. The addition of alum to bread has long been a thing of the past.

The recent investigations into the causes of "strength" in flour, particularly those of Wood carried out at Cambridge, have shown that the quality rather than the absolute quantity of the gluten is the factor of prime importance. Quality in gluten is mainly determined by the proportions of mineral salts present, not in the wheat as a whole but in the actual flour. There is reason to believe that flours differ among themselves both in the absolute amount and in the quantitative composition of their mineral salts. The mere addition of such mineral salts at the moment of doughing flour for bread-making has but a small effect—the necessary minerals must be introduced at a far earlier stage, either into the wheat itself by appropriate conditions of culture on the farm or at the time of manufacturing the flour from the wheat. The first alternative cannot yet be realised in practice; experience has proved, however, that favourable results can be obtained in the second way indicated. There will be but one opinion with regard to the nature of the substances that may with safety be added to flour in this way. It is essential that they should be materials natural to wheat but which Nature, owing possibly to vagaries of climate or of soil, has not provided in sufficient quantity or in the right proportion for the particular crop. The number of possible additions is thus very much restricted. Patents have been taken out for adding a variety of substances to flour most of which are indubitably of improper nature and most of them must have been proposed by persons of the inventor class entirely ignorant of what they were doing; the only substances whose use has been commercially successful are water, malt-extract and certain phosphates.

Both wheat and flour, far from being materials of constant composition, vary very considerably with regard to the amount of moisture they contain. Flours milled in hot countries or even milled here from the wheats grown in hot countries contain considerably less water than flours milled in England from English wheats. In consequence, flours vary considerably in their water-absorbing capacities.

Further, both wheat and flour are not inert substances; when they are stored, subtle changes brought about by the agency of enzymes are taking place continually. These changes are known to the miller as conditioning. Water must be present for the enzymic changes to happen and the presence of a little

FIG. 1.—Made from Southern Plate wheat, 1908–9 crop.

Illustrates effect of adding 0·2 per cent. malt extract to this flour. X = untreated; F10 = treated. Flavour of F10 very greatly improved. The malt extract suspended in water equalling 1 per cent. of the flour and sprayed in.

FIG. 2.—Made from Southern Plate wheat, 1910–11 crop.

No. 1. "Tin loaf" made from wheat dry cleaned. Flour contained 11·5 per cent. of moisture.
No. 2. "Tin loaf" made from wheat conditioned and flour conditioned also. It contained 15·67 per cent. of moisture.

Photography has failed to record the full difference in colour, visible to the eye, between the loaves but it is sufficiently striking as it is.

more or a little less water in flour may have a marked effect in facilitating or restricting enzymic action.

Commercially, water is added to flour in the form of a very fine spray, usually during the milling process, though the addition may be made also to the finished flour. Some wheats are in no way improved; in the case of others there is a very decided and definite improvement in the baking qualities of the flour. In the light of our knowledge of the factors influencing strength, this improvement is logically interpreted as indicating a change in the mineral constituents of the flour. That this is what actually happens will be shown immediately.

According to the report, spraying adds 1 or 2 per cent. of water to the flour and to this extent is a gain to the miller.

At the recent meeting of the British Association, Mr. A. E. Humphries described experiments showing the effect of spraying on the Southern Plate wheat of the 1910-1911 crop. The improvement produced in the loaf by such treatment is very striking, as will be obvious, without further explanation, on reference to figs. 1 and 2, for which we are indebted to Mr. Humphries; a full account of his observations is to be found in *Milling* of Sept. 9, 1911.

The practice of adding phosphates to flour is justified by Wood's analyses showing that the ash of strong wheats is richer in phosphates than that of weak wheats. There is no doubt that such additions cause marked improvement in the baking qualities of the flour and in the nature of the loaf made from it. Flours which already contain sufficient phosphate naturally are not improved by the treatment. Mr. Humphries has contrasted the behaviour of the 1909 and 1910 crops of No. 2 Northern Manitoba wheats in this respect—the former was much benefited by the addition of a little malt extract and ammonium phosphate, the latter showed no change after treatment. No more striking illustration of the effect produced by different climatic conditions could be desired.

The addition of phosphates to flour is made on the assumption that phosphorus is present naturally in the form of an inorganic phosphate. Dr. Hamill contends that this is true only of the ash of flour and that a large proportion of the phosphorus is present in organic combination and therefore possesses a special dietetic value. Our knowledge of this question urgently needs extension, particularly with reference to the question

whether such materials as phosphorus and iron are of use to the system when taken in an inorganic form. There is a growing tendency to answer this question in the affirmative and to discredit the older view to the contrary.

Since Dr. Hamill's report was issued, Mr. Humphries has announced at the British Association meeting that during the process of conditioning flour and still more so during the manufacture of bread from it, there is an actual conversion by enzymic action of organic into inorganic phosphate, so that finally 40 per cent. or more of the phosphorus is in the form of inorganic phosphate. If this be confirmed—and there is no reason to distrust the observations—Dr. Hamill's objection to phosphates as improvers can no longer be maintained.

The fact that inorganic phosphates are formed during conditioning affords a clue to the changes already alluded to which take place on spraying with water and explains their effect. Dr. Hamill seems to have quite overlooked the fact that flour is a changing substance or at least to have ignored it in his report. It is perhaps advisable to point out that the inorganic phosphates, such as potassium phosphate, present in wheat, differ very materially from the organic compounds; in some of these the phosphorus atom is united to compounds of carbon and nitrogen in groups of unknown complexity, whilst in others the organic complexes form ethereal salts of phosphoric acids.

Dr. Hamill gives two other reasons for prohibiting the use of flour improvers. The first relates to protein—his argument being that if by means of improvers weak wheats with little protein are made to simulate strong wheats the consumer gets a loaf containing less protein and consequently of lower nutritive value. This point was fully dealt with in the last article and the small differences in the amount of protein shown to be of but little importance.

The second statement is that the consumer loses by the treatment, as he gets a larger loaf containing more water from a treated flour and therefore there is a diminution of the actual amount of flour in each loaf. Quite apart from the fact that the bread of the larger loaf is certainly the more digestible and that thereby any small loss of weight would be more than made up, it is also probable that the untreated flour would be unsuited for bread-making by itself and if not blended with a stronger wheat could only be used for some other purpose.

Commercially it is an accepted fact that any advantage gained by a manufacturer of a competitive article as the result of a new process is very soon handed on to the consumer. Competition in industries like milling and baking is far too keen for any other alternative to exist. The use of improvers will render a very much larger quantity of flour available for making bread of the type demanded in this country and the consumer cannot but fail to profit by the practice.

It has been repeatedly stated in these articles that English wheats are weak in character: in other words, they give flours containing a small proportion of protein and these have a low power of absorbing moisture, partly because they already contain so much, so that when used alone they produce small close loaves; in consequence, their use is restricted and the price paid for them very low.

The Home-Grown Wheat Committee has been working during several years past to better this state of things and has striven both to increase the quantity and also the quality of the gluten. Climatic conditions naturally cannot be altered—their effect is seen in a season like that which has just passed, the wheat being materially drier and stronger than it has been during many years past.

The introduction of other varieties of wheat into cultivation in this country, such as the Red Fife used in Western Canada and America, is attended with economic difficulties on account of the small yield of grain and straw; though individuals have been successful in growing it, farmers as a whole will not use it, finding the weaker wheats more profitable. Actually the same economic considerations are making themselves felt in Canada and the United States and there is an increasing tendency to grow weaker wheats the world over.

Incidentally, reference may be made to the work of the Agricultural Research Institute, Pusa, where the selection has been made by breeding on scientific lines of a wheat combining the qualities of both strength and good yield suitable for growth in India. Indian wheat as it comes to the English market is characterised as being weak and soft and by its extreme dryness. It appears that wheat of this type is grown as the result of fashion specially for export but that the natives, like ourselves, much prefer to make their bread from a stronger flour and cultivate strong wheats accordingly. Some of these

native strong wheats have been selected for improvement and it would now appear to be possible for India to grow strong wheats for export purposes which will command a higher price and so give a greater return per acre to the cultivator.

Possibly, in the future, the public fashion in bread will change in character to meet the altered circumstances—in the meantime the strongest wheats are bound to increase in cost. If by the addition during milling of minute traces of substances natural to flour we are able to increase the strength of some wheats, there seem to be the very strongest economic grounds for supporting the practice. It has been shown that at bottom wheats of all kinds are practically of equal value in a mixed diet and it is difficult to see how any other objection to a proper way of improving any of them can be substantiated.

The question of bleaching must be considered separately. If it be carried out in a proper manner (electrically), the effect is to add nothing deleterious and only to produce a very, very minute change at most, whilst the appearance is improved and the palate helped. As before said, the question is one of ethics and cannot well be argued on any other grounds.

The milling industry is clearly alive to the importance of the questions raised by Dr. Hamill's report, as will be obvious when the following resolutions adopted at a recent general meeting of the Incorporated National Association of British and Irish millers are considered:—

"(1) That in the opinion of this association wheaten flour sold as such without any qualifying designation should be the unbleached and untreated produce of properly cleaned and 'conditioned' wheat only.

"(2) That a Board of Reference consisting of several highly qualified physiologists, chemists and business men should be appointed by the Government to consider—

"(*a*) Whether flour may properly be submitted to bleaching processes.

"(*b*) Whether flour may be treated with substances which are not 'foreign' to wheat, flour or bread;

"(*c*) If so, to determine what substances are natural to wheat, flour or bread and within what proportions such substances are permissible;

"(*d*) To consider the desirability of adding substances as

'yeast foods' and to determine what may be used as such and within what proportions they may be used;

"(*e*) To consider under what designation or designations flours may be sold for commercial purposes.

"(3) That the Board of Reference should from time to time review its conclusions and make new or revised recommendations."

The attempt has been made in the foregoing account to give some idea of the complexity of the problem to be considered; the arguments used may serve to show that there can be little doubt that a Board of Reference such as is suggested presents the only rational way of arriving at a solution that will be satisfactory to the public.

The Nutritive Value of Bread

It is fortunate that Dr. Hamill's report on this question has been issued at a time when mature reflection has shown the absurdity of the sensational outcry against white bread. That the standard-bread craze never reached the proportions its advocates would have us believe is made evident by the statement of our leading millers that the output of standard flour never reached 5 per cent. of the total. The report must be a disappointment to the advocates of reform, as the contentions of the reformers are one by one shown to be either fallacious or misleading and the claims advanced in favour of entire wheat flour are finally narrowed down to a single possibility based on experiments made by Dr. Hopkins with rats—and rats are not human beings.

In the main, the report follows the same lines as the earlier article in Science Progress and it will suffice to lay stress on those points which may now be considered as authoritatively settled.

Dr. Hamill realises that the problem of the nutritive value of bread is essentially a wide one and that it cannot be elucidated without taking into consideration a large number of separate factors.

In the first place an explanation is given of the various grades of wheat flour and of the process of milling, care being taken to define the technical terms used somewhat precisely. All the information available—mostly from American sources—

as to the nutritive value of various milling products and of the bread made from them is collected in tabular form.

The issue in the case of bread may be narrowed down to two questions: first, is there anything scientifically wrong in the present white bread; secondly, are any of the alternative breads suggested in any way better and more nutritious? Both questions are answered emphatically in the negative, with the one caveat that there are possibly certain constituents present in bread made from entire wheaten flours which render them of special value.

In defence of white bread, Dr. Hamill contradicts the favourite statement of the reformers that bread made from patents flour is merely a starchy food and agrees that the strongest patents flour is actually the richest in protein constituents. It is to be hoped that now this fact is upheld by Government authority, the public will no longer be misled.

He supports the views put out in Part I. of this article that no real indication of the nutritive value of bread is given by the protein value deduced empirically from the amount of nitrogen present and that deductions as to the nutritive value of the mineral matter derived from the determination of ash are equally worthless. It is an advance to have these facts clearly stated.

The many difficulties and uncertainties caused by disturbing factors of all kinds when attempts are made to determine the action of the digestive juices and the manner in which absorption and assimilation of the products of digestion of bread take place are fully realised: much remains to be done before any conclusions of value can be arrived at. It is important in this connexion not to overlook such factors as that of individual preference. Whiteness is instinctively associated with cleanliness and is therefore attractive to the palate. Snyder has stated that during the course of his digestive experiments, when the subjects were for some time restricted to a diet of bread and milk only and were suffering from the severest monotony of diet, there was the very strongest natural preference for white bread.

Physiologists agree as to the importance of avoiding monotony in diet—a diet consisting wholly of bread is not only undesirable but probably suffices only for the bare maintenance of life.

The evidence as to the effect of the presence of branny particles, even when they are very finely divided, on the digestibility of bread need not be repeated. Dr. Hamill supports the conclusion that bran is not only itself resistant to digestion but also has the property of causing other materials eaten at the same time to be less perfectly digested. He is also unable to confirm the supposed advantages entire wheaten bread possesses in supplying fluorine to the teeth or in preventing dental caries.

The case for entire-wheat bread is thus narrowed down almost exclusively to experiments made by Dr. F. G. Hopkins, in which it was shown that young rats fed only on white bread did not thrive so well as those fed on wholemeal bread and that the addition of acetic acid extracts of the wholemeal bread to the white bread improved the condition of the rats fed on white bread. Dr. Leonard Hill has quite recently confirmed this result by feeding rats on white and on entire-wheat bread; those fed on the former were found to thrive less satisfactorily but the addition of a little germ to the white bread removed the discrepancy. Experiments with pigeons made at Liverpool gave a similar result. Various interpretations have been put on these results. Leonard Hill attributes the value of the germ to the presence in it of certain amino-acids essential for growth. Dr. Hopkins's results have been ascribed to a deficiency of phosphorus compounds in white flour. He is inclined to take a bolder step and to ascribe the value of bran to the presence of unknown substances of complex nature which are neither enzymes nor hormones of the nature of the "Secretin" studied by Starling. Their presence enables the system to make full use of the tissue-building elements of the grain—milling apparently removes them to a great extent from fine white flour.

Until further information is available criticism is impossible. But Dr. Hopkins's objections to white bread must disappear at once when it is consumed with natural food, such as milk or meat, which contain the mysterious bodies unchanged; his preference for standard bread in a mixed diet is therefore of academic rather than of economic importance.

Dr. Hamill adds some useful evidence by summarising recent work on Beri-beri, a disease caused by a diet consisting of decorticated rice alone such as is sometimes customary in the East.

Beri-beri does not occur when the offal, amounting to about 10 per cent. of the grain, is consumed with the rice. Schaumann attributes the disease to an insufficiency of organically combined phosphorus; experiments he has made with animals appear to show that whilst the addition of inorganic phosphates has no effect on the disease, substances rich in phosphorus organically bound prevent or cure it. A search for the active principle in rice offal showed that 85 per cent. of the total phosphorus in the offal is ineffective and narrowed the effective agent down to substances soluble in alcohol, amounting to only 1·6 per cent. of the grain. The nature of the active substance is not known: it remains a most remarkable fact that the addition of this small proportion to rice deprived of its cuticle is efficacious in preventing the onset of the disease.

Work with wheat on similar lines will perhaps lead to interesting results. Taking Schaumann's and Hopkins's work into account, it would seem that particular combinations of elements of which perhaps phosphorus is an essential constituent may be necessary for the normal nutrition of the body.

A new field of inquiry is thus opened up for workers in nutrition.

The results quoted certainly justify Dr. Hamill's final finding, which may be quoted verbatim:

"Entire wheat flours (including stone-ground flours and 'standard' flours) are in nearly the same position as 'households,' although when made from weak wheats they will usually contain less available protein than 'households' made from strong wheats. They possess, however, additional constituents, due to the presence of branny particles and the germ of the wheat, which appear to have a value of their own in nutrition and may as a result of further investigation be shown to comprise phosphorus containing organic compounds or other substances the presence of which in some part of the dietary, even in minute quantity, is important in maintaining good health."

But it is realised that, after all, the question of bread is mainly an economic one. In the case of an average liberal mixed diet, the advantages which one variety of bread may possess over another become negligible. The extra cost of the proprietary breads more than counterbalances any superiority in nutritive value, far better value being obtained by applying

the difference in cost to the purchase of other foods. Children whose food consists largely of bread are in reality underfed in respect of not a few of the essentials of nutrition; it will be far better to supplement the bread by other foods such as milk than to substitute another form of bread.

In conclusion, the Local Government Board and Dr. Hamill are to be congratulated on the presentation of these reports. They will be of lasting value. Such criticisms as they have been exposed to in this article are written with the desire of arriving more nearly at the truth and in deprecation of any settlement based on sentimental considerations.

THE ACTION OF GRAVITY UPON THE MOVEMENTS OF AQUATIC MICRO-ORGANISMS

By HAROLD WAGER, F.R.S.

Last July, while taking a country walk, I stopped to look at a large shallow pond which took a bright green colour from a multitude of living particles charged with chlorophyll. The pond was about six yards across, perhaps a foot deep at the centre and very shallow towards the margin. The sun shone but was frequently obscured by small clouds. On stepping close to the brink, the water seemed to be a dense and uniform green liquid but a minute or two later it was seen to change its character. A green sediment seemed to be settling down to the bottom, not uniformly but in irregular threads or streaks, which before long rested on the mud, leaving the water clear and colourless. The sediment was not spread out equally but formed a coarse network, with meshes three or four inches in diameter, much smaller, however, in the shallow parts of the pond. This sinking of the green particles invariably took place during the passage of a cloud over the sun; when the sun broke out from the cloud, they slowly rose to the surface again. Descending particles were instantly arrested by the reappearance of the sun and began to move towards the surface. The meshes were most apparent during sinking or after settling to the bottom. In a faint light the green streaks showed a tendency to extend themselves transversely but in a bright light they took a direction more nearly parallel to the rays. I spent an hour or so by the pond, watching the rising and falling of the particles, as well as the alternate formation and dissolution of the network and satisfied myself that the changes constantly taking place were regulated by the varying intensity of the sunlight. The green organism present in this pond is known as *Chlamydomonas*; it is closely allied to the *Volvox* familiar to naturalists ever since the time of Leeuwenhoek.

The example just mentioned was not the starting-point of my inquiries, which had been carried on during some years previously.[1] I bring it forward merely as a particularly clear case of the behaviour of minute, green, aquatic organisms when acted upon by light of varying intensity.

Any observer whose attention has been drawn to the phenomena will be likely to put to himself such questions as these:

By what means are these living organisms enabled to rise and fall in the water?

What causes or enables them to arrange themselves in a network?

If, as is probably the case, the organisms derive some benefit from their peculiar movements and mutual arrangements, what is the nature of the benefit?

These and like questions can only be answered satisfactorily by experiments so conducted that the influential conditions can be modified one by one. *Euglena viridis*, a unicellular organism, which often abounds in stagnant waters rich in organic matter, has been found particularly suitable for the investigation.

Under ordinary conditions, Euglena is a small free-swimming organism, spindle-shaped, with an obliquely truncated fore end and a pointed hinder end. The cell-protoplasm contains chlorophyll-bodies which may be scattered uniformly except for a clear space at the fore end or they may radiate from the centre, leaving both ends free. Grains of a substance (paramylum) resembling starch but not coloured blue by iodine may also be found anywhere in the cell; they are generally more numerous in the neighbourhood of the chlorophyll-bodies. A nucleus is usually seen near the hinder end of the cell. There is a mouth on the under side of the fore end; this leads by a narrow pharynx to a large internal cavity. A pulsating vacuole is present and there is a conspicuous red eye-spot on the dorsal side of the pharynx. Locomotion is effected by a single long flagellum, which—being at the anterior end—carries the body forward by striking the water obliquely. The organism revolves on its longitudinal axis as it travels, taking a well-marked spiral path. Euglena, like all green plants, is

[1] See Wager, "On the Effect of Gravity upon the Movements and Aggregation of *Euglena viridis*, Ehrb. and other Micro-organisms" (*Phil. Trans.* B, vol. 201, 1911).

nourished by the decomposition of carbon dioxide and also by the absorption of dissolved organic matter. It is doubtful whether solid particles are taken in and digested.

Euglena abounds in farmyards, where it forms a bright green scum on the surface of dirty pools; it is also plentiful on sewage-farms.[1]

Like Chlamydomonas, Euglena is attracted towards a source of light. If a little mud containing this organism be put into a saucer of water and set in a good light, the green cells slowly emerge, forming at first a green film which overspreads the surface but afterwards rises through the water towards the light. When shaded, they begin to sink, arranging themselves at the same time in a network (fig. 1); it is enough to bring the hand between them and the light to set up these changes. Remove the hand and they move upwards towards the light, while the network disappears. In a strong light, which strikes the surface of the water obliquely, the Euglenæ form streaks more or less parallel to the rays; if the light be weak, the streaks are transverse. When placed in a glass vessel exposed to a strong light, the cells cluster on the best-illuminated side of the vessel and form a green line at the water's edge. From such a green line or any mass of Euglenæ floating at the surface, streams of cells may be seen descending into the liquid and gradually becoming disseminated through it. Being thus brought out of the shadow of the mass and into the path of the light-rays, they begin to ascend again and to seek the illuminated side of the vessel. A continuous circulation is thus kept up and the Euglenæ are prevented from too dense an aggregation, which would interfere with their assimilating and respiratory functions. Screen the vessel by a sheet of card and a downward movement becomes more decided, the Euglenæ disappear from the surface and the upper layer of the water becomes clear. If the vessel be kept in the dark the Euglenæ remain in constant movement, forming an irregular, diffused network which extends through half an inch or three-quarters of an inch of depth measured from the bottom

[1] Our limits of space preclude the possibility of discussing the life-history of Euglena at length. Some information concerning the encysted resting-stage, the means of dispersal and the behaviour of Euglenæ in the presence of adverse conditions, such as drought or the attacks of parasites, will be found in the original memoir (Wager, *Phil. Trans.* B, vol. 201).

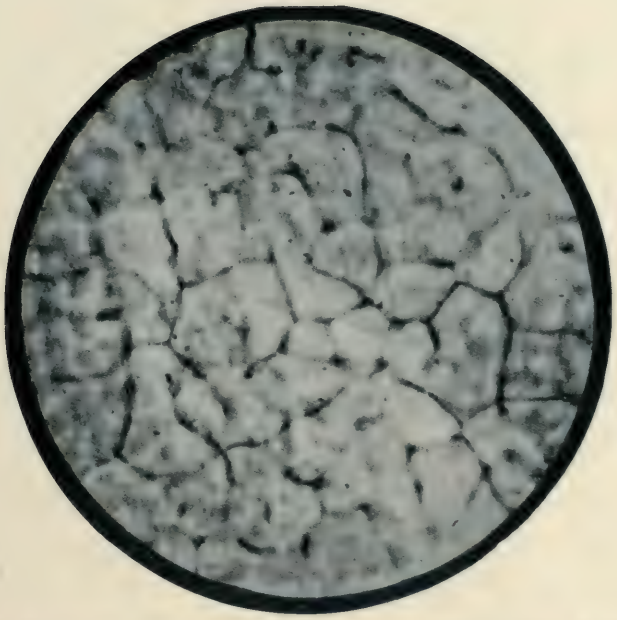


FIG. 1.—Living Euglenæ forming a network in a vessel about half an inch deep. (From *Phil. Trans.* B, vol. 201.)

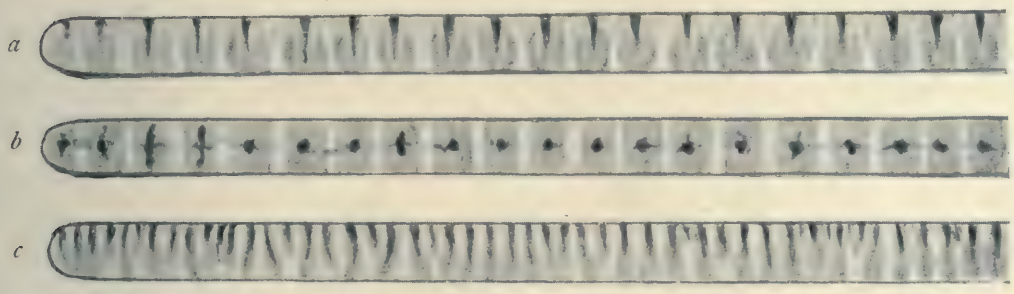


FIG. 2.—*Euglena viridis*, in a tube about 7 mm. in diameter placed in a horizontal position.

a, seen from the side; *b*, the same tube from above; *c*, shows an earlier stage of the aggregation. (From a drawing, natural size.)

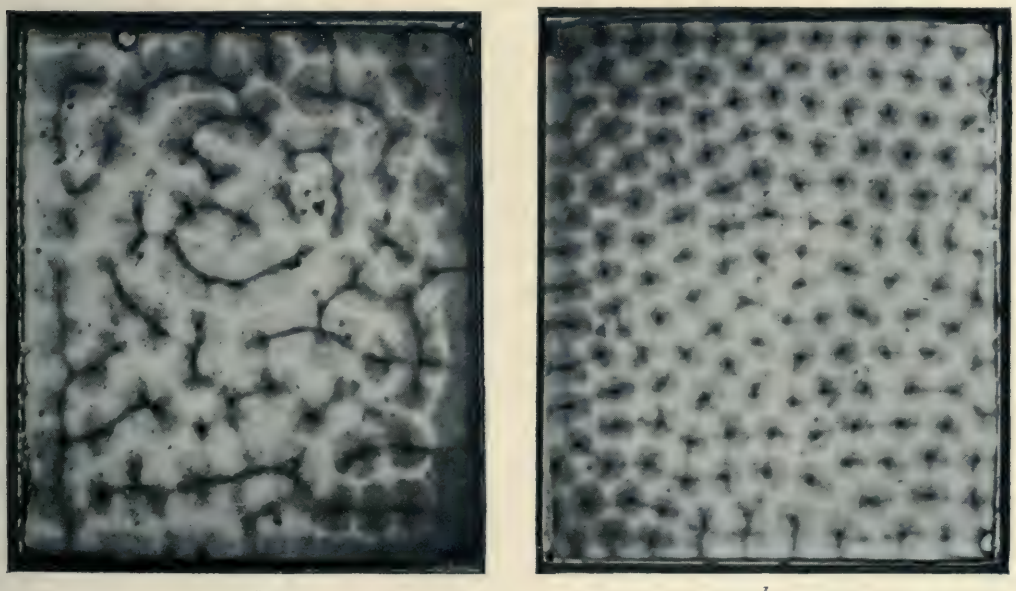


FIG. 3.—From photographs, natural size, showing the aggregation of *Euglena viridis* in a glass vessel $\frac{5}{16}$ in. deep placed in a horizontal position in the dark.

a, shows the formation of the network two and a half minutes after being placed in the dark; *b*, shows the breaking up into separate groups four minutes later. (*Phil. Trans.* B, vol. 201.)

of the vessel. Some interesting details are revealed by the study of Euglenæ enclosed in narrow glass tubes or shallow glass vessels.

If a tube about 25 cm. long and 7 mm. in diameter filled with water containing Euglenæ in sufficient numbers to give a pronounced green colour be placed in the dark in a horizontal position, in a little while the Euglenæ will be found to be no longer evenly distributed through the tube but aggregated, first into a rather irregular network (fig. 2 *c*) and a little later into a uniform series of vertical bands (fig. 2, *a b*). Exposure to light quickly breaks up the bands by causing the Euglenæ to travel towards the light. Under a low power of the microscope each band is seen to consist of a central dark mass of cells sinking with their hinder end downwards, whilst in the lighter-coloured peripheral area are many cells swimming

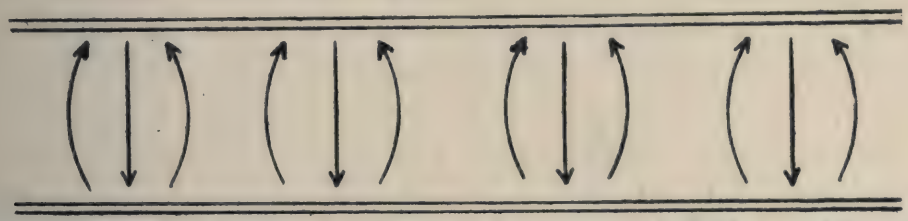


FIG. 4.—Diagram showing the direction of movement of *Euglena viridis* when regularly spaced in groups as shown in figs. 2 and 3.
The direction of movement in each group is indicated by the arrows.

upwards. The diagram (fig. 4) shows the direction of motion in these groups.

A flat-bottomed glass vessel, say 6 mm. deep, may be completely filled with water tinged green with Euglenæ, covered with a glass plate, sealed with vaselin to exclude the air and put in a dark place. A coarse network forms quickly (fig. 3 *a*), which will in a few minutes appear to break up into evenly spaced circular discs, each with a dark-green central spot or streak of irregular outline (fig. 3 *b*). The circular discs are the ends of cylinders and the central spot consists of a relatively dense mass of Euglenæ all sinking; the peripheral mass of the cylinder consists of Euglenæ more widely spaced, all moving upwards (see fig. 4). The grouping may persist during several days if the external conditions remain unchanged. This simple experiment shows that neither the aggregations nor the up-and-down movements of the Euglenæ originate in the action of light, by which, however, they may be greatly affected.

It seemed possible that convection currents might be concerned but both the aggregation and the movements were perfectly maintained in vessels kept at a uniform temperature and close to vessels of water containing light particles of dead matter which gave no sign of convection currents. Currents due to evaporation were precluded by the sealing of the vessels.

It is well known that under certain conditions micro-organisms respond to the action of gravity. Thus it has been found that they may rise to the surface of a liquid (a phenomenon which has been described as *negative geotaxis*) or sink to the bottom (*positive geotaxis*). The terms imply that the organisms do not respond to gravity simply because they are heavier or lighter than water but by reason of some kind of sensibility on the part of the organisms which enables them to respond to the gravitational stimulus.

In discussing the orientation of a motile organism under the influence of gravity, it is important to consider the purely mechanical possibilities due to density, structure and shape and the position of the centre of gravity. If the organism be heavier than the liquid in which it lives; if the position which it takes up in moving downwards be the same as that which it assumes when dead; and if this be in opposition to its normal direction of movement, it is fair to conclude that the downward movement is mechanical and not geotactic.

Now, Euglena is found to exhibit all these characteristics. It is distinctly heavier than water. The density was determined by means of solutions of gum arabic, varying in strength, in which it was found that Euglena can live for several days. At first they seem to be paralysed but in a short time many recover their mobility. In solutions of density below 1·014 they begin to sink at once; when the density is from 1·014 to 1·015 they sink more slowly; at 1·016 very few sink at once, the majority forming a distinct green layer at the surface, which after a time begins to sink. At 1·017 and 1·018 the majority do not sink at all; and at 1·019 all remain floating for an indefinite time. The density of *Euglena viridis* may therefore be set down as between 1·013 and 1·019; the average density as about 1·016.

Live Euglenæ, when sinking in water, take a vertical or slightly slanting position, the fore end, which goes first in voluntary movement, being turned upwards. Euglenæ killed by hot water or osmic acid take the same position when sinking

in water. This is what one would expect from the structure of the organism. Though the anterior end is the larger and would therefore tend to turn downwards in a medium of less density than the cell itself, the middle and posterior regions are probably denser, owing to the inclusion in them of the chlorophyll-bodies, the paramylum-grains and the nucleus, whilst the density of the anterior end is very likely lowered by the pharyngeal cavity and the pulsating vacuole.

The downward movement appears therefore to be the result of the mechanical action of gravity upon an organism heavier than water and does not necessarily involve anything of the nature of a geotactic response.

The mechanical action of gravity operates only when the organisms are crowded and by *crowding* I mean that many of them are only separated by about their own breadth.[1]

Euglena can be seen by the microscope to swim in the direction of its axis along a more or less spiral path. Gravity tends to bring it into a nearly vertical position but it is able by its own exertion to resist a mere gravitational pull. If, for example, Euglenæ which are swimming in a horizontal direction towards the light are suddenly shaded, the forward movement is slowed and they swerve into a more vertical position.

A similar effect is observed when Euglenæ become crowded, especially in the dark or in a diffuse light. If they are much crowded, their active movement is inhibited; the action of

[1] "That the downward pull of gravity is not effective until the organisms have become congested into a heap is beautifully demonstrated by the following experiment: A small number of motile Euglenæ are placed in a shallow cell, which is completely filled with liquid and sealed up with a cover-glass and vaselin. This is now placed under the low power of a microscope placed in a horizontal position [the stage being vertical]. On examination the organisms will be seen moving about freely, if the light is not too strong or if red light is used; no special tendency to move either upwards or downwards will be noticeable. If a bright spot of light is concentrated upon the middle of the cell from the source of illumination, which can be most easily done by manipulating the substage condenser, the Euglenæ will be attracted by it and will in a very short time become congested in the light space. If now the condenser be moved so as to take away the bright spot, leaving the field in a diffuse light only, the congested mass will be at once acted upon by gravity and begin to fall. . . . When they have begun to fall, their downward movement can be stopped almost at once by bringing back the bright spot of light into a position just below the falling stream; a few will pass beyond it at first but in a very short time the streaming will be stopped and they will again begin to congregate in the light space" (Wager, *Phil. Trans.* Series B, vol. 201, pp. 369–70, 1911).

gravity causes them to take the vertical position and they begin to sink. If they are less crowded, they may retain the power of movement but being compelled by the pull of gravity to assume the vertical position, they will be obliged to move upwards.

If a drop of water crowded with Euglenæ be introduced by a pipette into a vessel of water, the Euglenæ at once begin to sink; if they are thinly distributed through the drop, no downward movement will be observed and the organisms will swim about in all directions.

Or we may take a long tube containing water slightly tinged with Euglenæ and place it upright in a dark place; downward streams of Euglenæ will be set up here and there; they descend for a certain distance and then gradually disappear by the spreading out or diffusion of the organisms in the water. This is due to the gathering of the Euglenæ into heaps upon which the pull of gravity is effective. If a large number of Euglenæ are present the downward streaming is very pronounced and may carry most of them to the bottom, where they remain moving up and down within a distance of from three-quarters of an inch to one and a half inch.

In the dark or in a faint light the movements of Euglenæ are apparently controlled by gravity alone; in a bright light the action of gravity is negligible and the organisms swim upwards, *i.e.* towards the light, even when crowded together and this in deep water, such as that in a pond. If the light be cut off or much diminished, gravity acts once more and they sink. This explains the sudden clearing of a pond when a cloud passes over the sun.

"The upward movement which always regularly follows the downward one is caused by the active movements of the organisms themselves. As soon as the congested streaming mass of cells approaches the bottom of the vessel, it begins to spread out and the Euglenæ become diffused in the water. The crowding becomes diminished and the result is that the Euglenæ are now once more free to move. But they are still too crowded for entire freedom of motion; their axial orientation is still more or less vertical with the anterior ends upwards; consequently they are compelled to move upwards. That they are not able to move in any other direction is due to the action of gravity. Freedom of motion in all directions is only possible when the Euglenæ are few in number and more or less isolated from each other. The force of gravity always tends to bring them into a vertical position and anything which impedes, even to a slight degree,

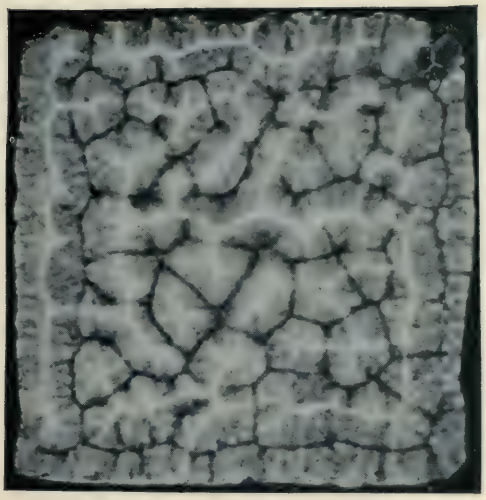
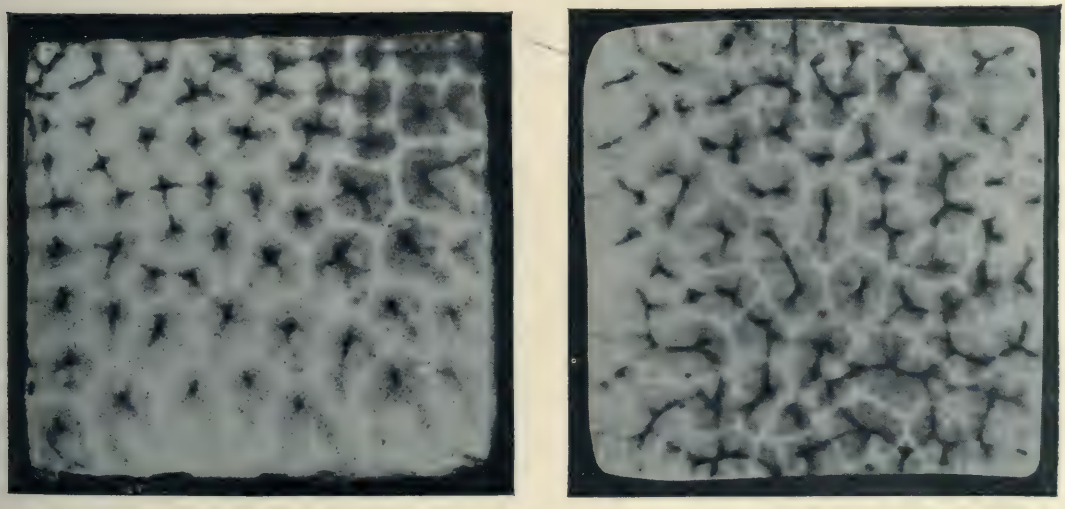


FIG. 5.—Aggregation of a precipitate of osmic dioxide in dilute glycerin.

As the precipitate sinks in the solution the particles become aggregated into a network almost exactly similar to that formed by living Euglenæ as shown in fig. 2. (From *Phil. Trans.* vol. 201.)



a *b*

FIG. 6.—From photographs, natural size, showing *a*, the aggregation of living Euglenæ into groups *b*, the aggregation, into similar groups, of a precipitate of osmic dioxide as it sinks in a solution of glycerin and water. (From *Phil. Trans.* B, vol. 201.)

their movements allows this action of gravity to be brought into play."[1]

"If there are few Euglenæ present in the liquid they may turn into a more or less horizontal position in contact with the surface film or, if the vessel is closed, with the solid body, cork, glass plate, etc., which may be used for that purpose. This contact reaction interferes, as Loeb[2] points out in the case of Paramecium, with the reaction to gravity. So long as the Euglenæ remain in this position, the action of gravity may be inhibited But if they are disturbed in any way, as by agitating the surface of the liquid with a glass rod or by shaking the vessel, they become once more oriented into a vertical position and if a sufficient number has accumulated they may sink again. This, no doubt, partly explains the disappearance of the motile Euglenæ from the surface of water which is observed in nature during rain or wind."[3]

We are now in a position to answer the first of the questions proposed on p. 299 by saying that both the upward and downward movements of Euglenæ are set up by gravity, modified in some cases by crowding; and further, that the action of gravity appears to be mechanical, involving no physiological response on the part of the organism.

Let us next consider the arrangement of Euglenæ into networks, neglecting the fact that they may at times be resolved into more or less separated groups. At first I thought it possible to explain the networks by differences in the food-supply or in the amount of carbon and dioxide and oxygen present.

"Euglenæ are certainly attracted to some extent by oxygen, as Aderhold[4] has shown; they tend to accumulate around bubbles of air in the water and their accumulation at the free surface of water in contact with air is, no doubt, partly due to this. I found, however, that the aggregation takes place just as well and as quickly in water which has been thoroughly boiled to get rid of the air as in water that has been well aerated, so that it could not be due to variation in the oxygen contained in the water. Nor is it due to air at the surface, for the aggregation is as perfect in closed vessels from which all air is excluded as in open vessels exposed to the air. That the

[1] Wager, *loc. cit.*, p. 370.

[2] Loeb, J., *The Dynamics of Living Matter*, New York, 1906.

[3] Wager, *loc. cit.*, p. 371.

[4] Aderhold, R., "Beitrag zur Kenntniss richtender Kräfte bei der Bewegung niederer Organismen," *Jenaische Zeitschrift f. Naturwiss.* 22, 310, 1888.

oxygen given off by the organisms themselves is not the controlling cause seems to be proved by the fact that the aggregation takes place best in the dark, when we may suppose the Euglenæ are not giving off oxygen. The presence or absence of carbon dioxide in the liquid appears to make no difference in the aggregation. Jennings[1] states that the crowding of Paramecium is due to the chemotaxis exerted by the excreted carbon dioxide. I have not been able to obtain any evidence that this is the case with Euglenæ. Various experiments tried with it gave purely negative results. Bubbles of carbon dioxide introduced into the tubes or vessels containing motile Euglenæ seemed to have no effect; there was no accumulation of Euglenæ around them as in the case of the oxygen bubbles and no modification in the aggregation. The only effect of the carbon dioxide that I have been able to detect is that in its presence the Euglenæ remain motile for a longer time than in ordinary air or oxygen, in which they tend to become rounded off."[2]

It is, moreover, very difficult to suppose that variations of this kind in the liquid would produce a regular grouping of Euglenæ and a persistent cycle of up-and-down movements.

"The fact that the organism is heavier than water and that the downward movements are due to the mechanical action of gravity suggested the possibility that the network-like grouping and aggregation might also be a purely physical phenomenon due to surface tension and cohesion and it occurred to me that the behaviour of chemical precipitates and other fine sediments of various kinds might afford some clue to an explanation of the phenomena.

"It was necessary, of course, to obtain a precipitate as nearly like the Euglenæ cells as possible as regards density and absence of a tendency to become flocculent. Various precipitates were tried, as well as sediments of different kinds, such as starch grains, dead cells of yeast and Chlamydomonas, fine coal dust, manganese dioxide, etc., all of which were useful; but the best results were obtained with dead cells of Chlamydomonas killed in osmic acid and a precipitate of osmic dioxide (OsO_2) produced by adding a dilute solution of ferrous sulphate to the ordinary 1 per cent. solution of osmic acid used for fixing. This precipitate does not become flocculent and does not

[1] Jennings, H. S., "Studies on Reactions to Stimuli in Unicellular Organisms. I.—Reactions to Chemical, Osmotic, and Mechanical Stimuli in the Ciliate Infusoria," *Journ. Physiol.* 21, 1897.

"Reactions to Stimuli in Unicellular Organisms," *Amer. Journ. Physiol.* 3 1899, 1900.

Behaviour of the Lower Organisms, New York, 1906.

[2] Wager *loc. cit.*, pp. 371, 372.

stick but always remains in a fine granular condition; it has, moreover, the advantage of being black, so that the figures formed by it can be easily seen. I got good results when this precipitate was allowed to settle in water but much better when it was allowed to settle in a denser liquid, such as a solution of one part of glycerine in four parts of water, by which its relative density to the glycerine solution was nearly equal to that of the Euglenæ to water."[1]

"If a flat glass dish, which can be covered with a glass plate to exclude air, similar to that used in experiments with Euglenæ, is filled with water containing a precipitate of osmic dioxide, the separate particles settle down in straight lines and form a thin film on the bottom. If the dish is now gently turned upside down, this film of particles, in a second or two, again begins to sink in the water but directly gravity begins to act upon them they become attracted into a network (fig. 5) almost exactly resembling the network produced during the Euglena aggregations under similar conditions (fig. 2).

"If the downward movement is retarded by the use of a denser solution, such as glycerine and water, the network which is at first formed (fig. 5) is not only much clearer but, under such conditions, tends to break up into separate masses or groups of particles, exactly resembling a later stage in the aggregation of Euglena. The resemblance extends even to the darker, denser central stream surrounded by a less dense peripheral layer. This less dense peripheral layer consists of a more or less well-formed vortex ring produced by the friction of the water. In the case of Euglena, the lighter area around the denser central mass is also of the nature of a vortex ring, but is not so clearly marked owing to the upward movements of the organism.

"Except for the fact that the cells of Euglena are capable of free movement, the aggregation of the particles of a precipitate appears to be similar to the aggregation of living Euglenæ."[2]

"The aggregation of these various sediments appears to depend upon the fact that a thin film of particles, together with the surface layer of water in which they are contained, acts as a liquid heavier than water. The aggregation is brought about therefore not by the action of gravity upon the separate particles but upon the mixture of particles and water as a whole, just as would be the case if we were dealing with a homogeneous liquid heavier than water."[3]

"The physical forces which are called into play during the aggregation of a layer of sediment in a closed shallow vessel

[1] Wager, *loc. cit.*, pp. 372–3.
[2] *loc. cit.*, pp. 373–4.
[3] *loc. cit.*, p. 374.

thus appear to be (1) gravity, (2) the adhesion of the sedimentary layer to the upper surface of the vessel, (3) the cohesion in the sedimentary layer, (4) the surface tension reaction between it and the water, and (5) the friction of the water.

"The network is therefore to be regarded as the resultant of these forces, and appears to be explainable as follows:

"When the film is turned upside down the force of gravity immediately begins to act upon it, and the first effect is that the adhesion to the glass plate or surface is weakened. It then begins to sink. As soon as this takes place the cohesion of the sedimentary layer asserts itself, and the particles come closer together in some places, and separate in others, the result being an irregular network. If the downward movement due to the fall of gravity is slow, as when a denser solution than water is used, the network is more regular in character, and as the particles descend in the liquid slowly, they are gradually attracted into more or less regular and equally spaced groups. These aggregations, therefore, appear to be cohesion phenomena, and the patterns produced are cases of cohesion figures."[1]

We can obtain similar cohesion figures in a variety of ways whenever we have a more or less homogeneous film placed under such conditions that a force or forces acting in opposition to the cohesion of the film or layer can be brought into play. The network-like aggregations of Euglena are comparable with those which are formed by precipitates or sediments and are probably to a large extent formed in the same way.

"A drop of water which contains a large number of motile Euglenæ or other micro-organisms behaves just as a liquid heavier than water, such as a drop of ink or of a solution of cochineal. If it is placed on the surface of clean water it sinks as a whole beneath the surface, expands into a vortex ring, which gradually becomes larger but is slowly broken up by the friction of the water and finally becomes diffused in it, just as a drop of cochineal would do or a vortex ring produced by a mixture containing a fine sediment.

"If the Euglenæ are much crowded but evenly distributed, either in an open or closed shallow vessel, the first effect of gravity is to cause them to sink. This brings them at once into closer contact with one another and a dense layer is produced, in which phenomena of cohesion are apparent in the formation of a well-marked reticulation.

"If the number of Euglenæ in the water is only sufficient just to give it a green colour, this downward movement does

[1] Wager, *loc. cit.*, p. 375.

not take place but the effect of gravity is, first of all, to cause an orientation of the cells in a vertical direction, with their anterior ends upwards. Their direction of movement is thus definitely determined and as they are not sufficiently crowded for the *downward* pull of gravity to be effective, they begin to move more or less directly upwards. As this takes place simultaneously in all parts of the dish, they soon become congested as they reach the upper layers; they lose more and more their freedom of movement and now the conditions are such that the downward pull of gravity becomes operative and the Euglenæ begin to sink. At the same moment the cohesive force comes into play and a network-like cohesion figure is formed.

"If the cell is a shallow one, the aggregation is more regular at the beginning and takes place more quickly than in a deeper one. This is explained by the fact that the upward movement brings them more quickly to the upper surface and that they form here, before cohesion has had time to take effect, a more continuous layer than is possible in a deeper cell, where the upward movement is more irregular and cohesion begins to take effect before they have formed a homogeneous layer."[1]

Our answer to the second question on p. 299 is therefore that Euglena networks are formed by molecular attraction or cohesion, set up, just as in a film of inert particles in water, whenever the Euglenæ become massed into a tolerably homogeneous layer, upon which gravity can act.

We may now attempt to answer our third question, as to the benefit which the organisms derive from their movements and aggregations.

Euglena is nourished in two different ways: (1) through its chlorophyll, as in a green plant; and (2) saprophytically or possibly in animal fashion by the absorption of organic food. Obviously, frequent insolation at the surface of the water would promote carbon fixation but the clustering of the motile cells at the surface must not be dense nor long continued. If it were, the organisms would obstruct one another, assimilation and respiration would be hindered and the organic food in the lower depths of the pool would be insufficiently utilised. The action of gravity obviates such disadvantages by causing densely clustered cells to sink and setting up a continuous circulation of the organisms, which are induced to move steadily from the depths to the well-lit surface and then from the surface to the

[1] Wager, *loc. cit.*, pp. 378–9.

depths. The advantage drawn from approximately uniform dissemination of the organisms through the pool will be appreciated by all who have been in the habit of considering the many natural contrivances which facilitate the spread of birds, insects and common weeds, thereby preventing the waste which would inevitably occur if some areas were regularly congested while others were but scantily populated.

THE CORROSION OF IRON AND OTHER METALS

Part II

In the former article on this subject (Science Progress, April 1911), it was contended that Moody's argument that iron rusts only in the presence of an acid is essentially correct and that the main precaution to be observed, if it be desired to prevent corrosion, is to avoid the accumulation of liquid films on the surface of the metal; it was also contended that the introduction of the conceptions and nomenclature of the modern physical chemist has not only done little if anything to advance our knowledge of the subject but has placed difficulties in the way of the practical man: to parade the mythology of ions—hydrogen and hydroxyl—and invoke the entire machinery of an imaginary "electrolytic dissociation," in such a case, when we can express ourselves in terms of simple acids and alkalies, serves no useful purpose.

As I have insisted repeatedly but with slight effect, it is essential that we should arrive without delay at clear conceptions of the nature of chemical change in general, especially in the case of metals; to this end, it is important that we should seek to introduce an element of common sense into our proceedings and no longer allow ourselves to be tied by narrow considerations. Logic demands that we should endeavour to discuss the problems before us from every possible point of view: as scientific workers, we shall have no claim whatever to consideration if we cannot be scientific to this extent; and practical men will continue to despise theory if those whose business it is to deal with theory show so clearly that they are the slaves of preconceived opinion and of narrow prejudice; we must on all occasions be prepared to question the grounds of our belief whatever the issue before us may be and whatever the consequences. In no case should we allow ourselves to be tied by an hypothesis; it is true, that

outside mere haphazard discovery—which has often been attended with the most important results—hypotheses are of infinite service to the inquirer, that there can be no rational inquiry without some hypothesis; yet the modern tendency to strain each particular hypothesis to the utmost limit and to confine attention to it alone, is to be deprecated on many grounds, especially because it so limits the ambit of experimental inquiry.

The recent May meeting of the Iron and Steel Institute, held since the previous section of this article was written, brought forth the now usual crop of papers on the corrosion of iron. In the interval a valuable work on the subject by Dr. Friend has also been published; in this he not only describes his own experiments but also summarises the very large number of observations that have been placed on record by various workers. It will be only too obvious to those who study this work and the papers referred to (p. 323) that the whole subject is still shrouded in a deplorable obscurity.

In point of fact, the subject of the corrosion of metals not only teems with unsolved problems but is one in which clear thinking is required, so that the fundamental issues may be rendered obvious and experiments instituted on proper lines.

At the outset we may write $C = E/R$, the well-known formula of the electrician—Ohm's law—as expressing the facts. In the case of any change, the amount of chemical change and therefore the electrical current, C, is proportional to the electrical pressure or chemical efficiency developed in the electrolytic circuit within which the change takes place and inversely proportional to the electrical resistance in the circuit. This statement by implication involves the acceptance of the view that the conditions which obtain in a voltaic cell are those that must be fulfilled if action is to take place. If it were generally taught that such an expression governs every change, some progress towards an understanding would be made. But it is not one of the articles of physical-chemical belief commonly promulgated: it is never put forward in the text-books—indeed, interactions are only too often spoken of as *either* chemical *or* electrical, as if it were possible to draw a distinction between the two kinds of change. As I have frequently taken occasion to point out, no heed whatever is paid in

these days to Faraday's early writings on the subject; if his pronouncements had been duly weighed the confusion that now prevails could not have arisen.

The ionic dissociation school has never yet put forward any clear statement defining precisely the conditions which will give rise to change: there is much talk of "solution pressure" but we are never told what determines the "pressure" nor are the conditions under which alone it comes into operation defined; the fact is usually overlooked that no action ever takes place independently not only of the solvent which bathes the metal but also of the electro-negative element with which it is coupled—the third necessary component in the system within which change occurs. The term is little more than a *façon de parler*—one of the many expressions coined in modern times which serve very usefully not only to cloak ignorance but to convey a false impression of learning. The conception is about as real perhaps as would be that of a tug-of-war pressure in a set of boys standing idle alongside the rope on which they will pull against one another when the game is played.

Ohm's law leaves the all-necessary third component out of account, except in so far as it enters into the value of R; in any case, it is clear that the part the electro-negative component plays has not yet received sufficient consideration. Perhaps progress in the applications of electricity depends to no slight extent on a proper understanding of the function of this factor: the successful solution of the great problem of electrical combustion — of combustion involving the direct development of electrical energy—may hang upon it.

It is well known that a single fluid cell of the old type is subject to constant polarisation and that different combinations in which a given positive element is associated with different negative elements in turn—for example, zinc with either copper or silver or platinum or carbon—give rise to different average electro-motive forces: the reason being probably that the extent to which polarisation takes place varies with the nature of the electro-negative material.

In the Grove platinum-hydrogen, platinum-oxygen, sulphuric acid cell, presumably both the oxygen and hydrogen are not merely in contact with the platinum plates but in some intimate way attached to or "alloyed" with them; otherwise it

is difficult to understand why so few metals can be substituted for platinum and why combustible gases such as methane and carbonic oxide cannot be coupled with oxygen in a gas battery.

Hydrogen apparently is peculiar in its relationship to platinum, peculiar in forming with it a hydride, perhaps of the binary class, such as is not producible from other metals, at all events by a direct process. Methane presumably has no such "combining power"; carbonic oxide, instead of forming binary compounds, apparently has a pronounced tendency to form "cycloid" compounds, as in the case of nickel carbonyl. In any case, some explanation is needed of the differences observed between combustible gases; it may well be that the differences are more or less of the order now suggested.

The results obtained by Lambert and Thomson, referred to in the previous article, merit discussion from this point of view. Assuming that acid (carbonic acid) was present together with the oxygen and water, the fact that the highly purified iron they prepared did not rust unless coupled with an electro-negative conductor may be regarded as proof that two contiguous portions of a mass of iron cannot respectively play the parts of the electro-positive and the electro-negative components of a voltaic couple in presence of a depolariser —in other words, that oxygen cannot act as a depolariser at or on a simple iron surface.

It has been customary to assume that when copper dissolves in dilute sulphuric acid in the presence of oxygen, different more or less adjacent parts of the metal form the positive and negative terminals and that at one of these the acid is active whilst the depolariser (oxygen) is active at the other.

It is of course conceivable, indeed probable, that there are impurities in copper which are active in promoting depolarisation. The difference between iron and copper, however, may arise from the fact that copper oxide has a lower and iron oxide a higher heat of formation than that of water. Or it may be that iron lacks the power of merely absorbing oxygen at its surface; iron, we know, is rendered passive with peculiar facility: apparently it is easy to "polarise" it permanently with oxygen.

When ordinary iron rusts, most of the hydrogen it displaces from whatever acid may be present becomes oxidised; the

impurities in the iron are more than sufficient to act in conjunction with the oxygen as the depolarising electro-negative element in the current of change.

As highly purified zinc is scarcely attacked by acids in presence of air, it would seem that in the case of this metal, as in that of highly purified iron, oxygen cannot act as depolariser in the absence of an electro-negative component in the circuit to condition change. I am not aware that in experiments such as were made by Reynolds and Ramsay with highly purified zinc, referred to in the previous article (p. 643), the attempt was made to exclude air.

In presence of an electro-negative conductor such as mercury, oxygen acts readily as depolariser of the hydrogen displaced by the zinc. Traube's experiment is well known in which a zinc-mercury alloy is shaken with lime water in a flask containing oxygen; the hydrogen displaced by the zinc is absorbed and an amount of hydrogen peroxide produced which is practically equivalent to the amount of zinc dissolved (cf. *Ber. deut. chem. Ges.*: **26**, 1471, 1893).

$$Zn + Ca\begin{Bmatrix}OH\\OH\end{Bmatrix} + \begin{matrix}O\\\dot{O}\end{matrix} = Zn\begin{Bmatrix}O\\O\end{Bmatrix}Ca + \begin{matrix}HO\\H\dot{O}\end{matrix}$$

In view of these conclusions, it is desirable to ascertain whether acids act on highly purified copper in presence of oxygen alone or whether, as in the case of iron and zinc, a relatively electro-negative conductor be required to condition change.

The question here raised is one of considerable practical importance; attention is called to it, however, chiefly in order to emphasise the need of taking into account the part played by the electro-negative component of the circuit in all cases in which the corrosion of metals is concerned. It cannot be too forcibly insisted that action only takes place when the presence of an electro-negative component is assured and that action takes place always and only in an electrolytic circuit.

It is remarkable and a striking illustration of the truth of my contention (p. 311), that Friend does not deal with the subject of "galvanic action" until the fourteenth chapter of his book is reached and that his treatment of the subject is vague and unsatisfactory. Throughout the earlier chapters he distinctly implies that iron will dissolve in acid even if pure; and that

this is his belief is clear from many passages in chapter xiv., such as the following, for example:

"It may be well, therefore, at the outset to make perfectly clear what should be meant by the term galvanic and then to try and determine in how far galvanic activity may be said to effect the rate of corrosion of metals (p. 238).

"Reverting to the simple cell composed of copper and iron plates immersed in dilute sulphuric acid, we see that, as the iron dissolves, chemical energy is set free; the chemical equation

$$Fe + H_2SO_4 = FeSO_4 + H_2$$

is therefore incomplete in so far that it takes no notice of this energy. When the iron is isolated, that is to say when it is allowed to dissolve in the acid, *not being in contact with any other metal* (my italics), this chemical energy is set free almost entirely as heat (p. 239)."

It is extraordinary that such loose doctrine should prevail even at the present day—that it should be possible to disregard fundamental principles so entirely: until clearer views obtain as to the nature of the initial change whenever iron is attacked, it will be useless to attempt to discuss the more complex issues. But the confusion obvious in the passages subsequent to that last quoted is even greater.

"The bulk of this heat is lost by radiation and in increasing the temperature of the sulphuric acid and the containing vessel. If we can so arrange matters that the loss by radiation is entirely avoided and the amount of heat absorbed by the containing vessel and acid in raising the temperature through each degree is reduced to a minimum, the actual rise in temperature will become relatively great, with the result that the amount of iron dissolved in unit time will be proportionately increased. In practice, however, the loss by radiation is so great, and the heat capacity of the surrounding medium so large, that in general no very great rise in temperature may be observed and the iron dissolves at a relatively uniform rate. Almost the same result as is obtained by raising the temperature may be arrived at by connecting the iron with the copper by means of a wire. Part of the chemical energy liberated by the solution of the iron is now converted into electricity which is not lost by radiation as was the heat, but stimulates the chemical action in a similar manner as an equivalent rise in temperature. Consequently the iron passes more rapidly into solution even though the temperature remains substantially the same. It follows

therefore that, whenever galvanic activity is induced, the total amount of corrosion is increased, although the actual extent of that increase must depend upon local circumstances" (p. 240).

It was demonstrated by Faraday that whenever chemical change takes place the energy liberated is in the form of electrical energy and we must regard it as established that heat is developed as a consequence of the frittering down of electrical energy by the resistance of the circuit. Whenever a proper path is provided, as in a voltaic cell, the electrical energy can escape as such almost entirely; if, however, the action take place in a closed circuit from which there is no outlet, as when a metal is simply dissolved in an acid, the electrical energy is apparent only as heat.

It may be asserted, I believe, without hesitation that whenever corrosion takes place it takes place in a "galvanic" circuit, and that galvanic activity necessarily accompanies corrosion.

In his book Friend discusses at length the question "Is an acid necessary to corrosion?" The conclusion at which he arrives agrees with that stated in the earlier part of this article—that it is. He is of opinion that previous observers who have contended that the presence of an acid is not necessary have failed to exclude carbonic acid from their apparatus. But although he regards Moody's experiments with favour, he ultimately discards them as evidence on the ground that Moody used chromic acid and thereby rendered the iron "passive"—overlooking the fact that in Moody's experiments the iron rusted at once when brought into contact with the glass of the tube in which it was enclosed and also when air containing carbon dioxide was admitted to the apparatus. The objection that the iron would be rendered "passive" by the treatment to which he subjected it was foreseen by Moody and disposed of by him. Friend's own experiments, to which reference has been made already, confirm Moody's result in the most striking manner possible.

It is commonly supposed that iron will not rust in presence of alkali. Friend in his book describes a series of experiments, which are a most important contribution to the discussion of this question, in which iron was kept in contact with purified oxygen and solutions of caustic soda: when he used a very

dilute solution (one containing 1·3 grammes of soda per litre) the iron rusted rapidly but not in stronger solutions. He accounts for this result by assuming that in making, filling and sealing up the apparatus he used it was impossible to exclude carbon dioxide entirely and that therefore the soda solution contained a certain amount of carbonate. He then calls attention to Küster and Gruter's observations showing that aqueous solutions of sodium carbonate are more or less hydrolysed in the sense of the equation

$$Na_2CO_3 + H_2O = CO_2 + 2NaOH,$$

the partial pressure of the carbon dioxide in equilibrium with a normal solution of sodium carbonate being 0·000072 atmospheres at 90° C. Friend supposes that in the case of the very dilute alkaline solutions he used sufficient carbon dioxide was present in the atmosphere to condition rusting.

When he used a saturated solution of caustic baryta in place of a strong solution of caustic soda in the apparatus described in the previous article, the iron always rusted in the course of an hour or two. He points out that the baryta was not specially purified and probably contained an appreciable quantity of carbonate in addition to that formed from the carbon dioxide in the flask: in fact, he supposes that the baryta was not capable of withdrawing every trace of carbon dioxide from the atmosphere—being less effective in this respect than soda.

Friend is therefore to be regarded as an out-and-out defender of the "acid theory of corrosion." In his conclusion he refers to the so-called "electrolytic theory" discussed in the previous article and points out that no proof has been given that Kohlrausch and Heydweiller had pure water at their disposal and that the minute degree of electrical conductivity which they observed water to have may have been and probably was due to the presence of a minute proportion of carbon dioxide. He concludes with the remark: "If such is really the case, the electrolytic theory of corrosion becomes a myth, whereas the acid theory is unaffected by it."

With reference to this conclusion, it may be pointed out again that to draw a distinction between an electrolytic theory and an acid theory is little short of absurd—the acid theory is necessarily an electrolytic theory: the only question at issue is whether or no water pure and simple can function as an

electrolyte and condition rusting in presence of oxygen. It is illogical to assume on the evidence advanced by Kohlrausch and Heydweiller that it can, as every step they took to purify water further involved a diminution of its electrical conductivity and it is therefore permissible to assume that ideal water—water pure and simple—would be a non-conductor; on general grounds it is also to be supposed that a binary compound such as water would not be an electrolyte. The conclusion that water is "electrolytically dissociated" is purely hypothetical; and all the arguments used in support of the hypothesis involve reasoning within a vicious circle. If chemists are wise they will cease to talk of the "acid" and the "electrolytic" theory of rusting as distinct "theories"; practical men will then no longer be justified in calling "A plague on both your houses" and in time may show some respect for theory that is brought under their notice in simple and straightforward language.

The Influence of Oxygen in promoting Corrosion

The rusting of iron is undoubtedly a complex process and rust may be formed in more than one way, according as the action takes place primarily either in a circuit from which oxygen is absent or in one in which it is present.

When iron—coupled with an electro-negative conductor—is brought into contact with an acid solution, it is dissolved as ferrous salt probably in the manner illustrated in the equation

$$Fe + \begin{matrix} CO_3H_2 \\ CO_3H_2 \end{matrix} = Fe \left\{ \begin{matrix} CO_3H \\ CO_3H \end{matrix} \right. + H_2$$

Ferrous bicarbonate

Or it may be—especially in the case of weak solutions—that a "basic salt" is the first product:

$$Fe + \begin{matrix} CO_3H_2 \\ OH_2 \end{matrix} = Fe \left\{ \begin{matrix} CO_3H \\ OH \end{matrix} \right. + H_2$$

Ferrous hydroxycarbonate

In any case, whatever the conditions, in so far as iron is dissolved at all by an acid, it may be supposed that the metal is always converted into ferrous salt; the production of rust may be said to be a secondary phenomenon.

Presumably when a ferrous salt in solution is brought into contact with oxygen it is converted into a ferric salt and rust

is formed by the hydrolysis of this salt. It is unnecessary to consider the precise manner in which the oxygen acts; the process may be exemplified empirically in the following manner:

$$2Fe\begin{cases}CO_3H\\OH\end{cases} + (O_2 + OH_2) = 2Fe\begin{cases}CO_3H\\OH\\OH\end{cases}$$

Ferrous hydroxycarbonate — Ferric dihydroxycarbonate

$$Fe\begin{cases}CO_3H\\OH\\OH\end{cases} + OH_2 = Fe\begin{cases}OH\\OH\\OH\end{cases} + CO_3H_2$$

Apparently rust is formed by the "condensation" of several molecules of ferric hydroxide, $Fe(OH)_3$—how many we do not in the least know but to judge from its colour and other properties the rust molecule must be regarded as a complex structure. In the process the hydroxide undergoes more or less complete dehydration.

The production of rust in this manner, *in solution* as it were, at a distance from the iron, may be demonstrated in a very interesting way, as Moody has shown, by packing the lower end of a glass cylinder with ordinary iron-wire nails, then covering the nails with water to the depth of an inch or two after placing above them a disc of white filter-paper. On observing the cylinder at intervals, it is seen that the nails undergo little if any perceptible change but that rust gradually separates out from the liquid and settles on the paper disc above them.

When oxygen is present together with acid in a solution bathing an iron surface it is included in the circuit of change and acts as *depolariser*; in this case no hydrogen is evolved. Initially, when no ferrous salt is present, the interaction that takes place is analogous doubtless to that which attends the dissolution of zinc in presence of oxygen.

$$Fe + \begin{matrix}CO_3H_2\\CO_3H_2\end{matrix} + \begin{matrix}O\\\dot{O}\end{matrix} = Fe\begin{cases}CO_3H\\CO_3H\end{cases} + \begin{matrix}HO\\H\dot{O}\end{matrix}$$

It is to be supposed that when ferrous salt is also present this is at least to some extent included in the circuit of change and converted into ferric salt; thus:

$$Fe + \begin{matrix}CO_3H_2\\CO_3H_2\end{matrix} + \begin{matrix}HO\\H\dot{O}\end{matrix} + \begin{matrix}Fe(CO_3H)_2\\Fe(CO_3H)_2\end{matrix} = Fe(CO_3H)_2 + 2Fe(OH)(CO_3H)_2$$

In other words, while iron dissolves as ferrous salt, ferrous salt which has been produced previously is converted into a basic ferric salt; in the final stage this latter salt is acted upon by water and converted into rust and acid, the which acid is then available to dissolve a further quantity of iron.

It is clear that when iron dissolves merely as ferrous salt the rate of change (the value of C in the expression $C = E/R$) must be less than when it is dissolved in a circuit in which oxygen is included and operative.

The heat of dissolution of iron (that given out on dissolving a gramme atomic proportion, *i.e.* 28 grammes), according to Thomsen is 24,840 gramme-deg. C. heat units; the corresponding value for chlorhydric acid is 21,310 units; the maximum electromotive force developed on dissolving iron in these acids is therefore

$$\frac{24,840}{46,000} = 0{\cdot}54 \text{ volt and } \frac{21,310}{46,000} = 0{\cdot}46 \text{ volt.}$$

Probably the value given by carbonic acid does not differ greatly from the second of these.

It is to be supposed that when, in presence of oxygen, the hydrogen displaced by iron in dissolving is oxidised effectively in the primary circuit (entirely to water) a large additional amount of energy is developed in the circuit, equal to about 68,000 heat units, so that the electromotive force is raised about one and a half volt beyond that of a single fluid iron-acid cell. The part oxygen plays is therefore a very important one in so far as it is included in the primary circuit in which corrosion is effected; at present, however, we cannot say to what extent it is included in that circuit and to what extent energy is wasted in secondary changes which take place outside the circuit of corrosion.

It is well known that when iron has once begun to rust in air corrosion proceeds more rapidly, the rust appearing to promote attack. There is little doubt that this is in some measure due to the fact that the finely divided oxide serves to attract and condense moisture. It has been supposed that the rust also promotes corrosion by its electro-negative character. It may be doubted whether ferric oxide itself has much influence on this account but inasmuch as the electro-negative impurities in steel are set free as corrosion proceeds

it is easy to understand that as these accumulate on the iron surface they may well exercise a considerable influence in lowering the resistance in the circuit of change by increasing the area of the electro-negative element.

Influence of Salts

On the assumption that iron rusts only when "acid" is present, it is not to be expected that strictly neutral salts will promote rusting; this conclusion appears to be a correct one. As Adie and others have shown, in the absence of air, solutions of the sulphates and chlorides of the alkali metals are without action on iron but it is more or less attacked by magnesium chloride, especially if the temperature of the solution be raised; magnesium chloride, it is well known, is more or less acted upon by water according to the temperature in the sense of the equation

$$MgCl_2 + OH_2 = HCl + MgCl \,.\, OH$$

and the solution is therefore in some degree acid.

The evidence as to the action of neutral salts when air has free access to the solutions is somewhat conflicting. In the case of concentrated solutions iron seems always to be less attacked than it is when exposed merely in water, the reason being apparently that such solutions contain less oxygen than is present in ordinary water. That corrosion occurs at all in such cases must be attributed to the presence of carbonic acid in the solutions.

According to experiments made by Heyn and Bauer, accounts of which were published in 1908–1910, even dilute solutions of the chloride and sulphate of potassium and sodium have less corrosive action on iron than distilled water alone. Friend and Brown came to an opposite conclusion but Friend points out in his book than an explanation of the difference is probably to be found in the fact they have established, that whereas at 10° C. certain solutions of sodium chloride have greater corrosive action on iron than distilled water at the same temperature, at 14° the solution and water have the same corrosive action, whilst at 21° the water has the greater action, as observed by Heyn and Bauer. It is to be supposed that the extent to which corrosion takes place will depend not only on the amount of carbonic acid but also on

the amount of oxygen present in solution; the balance must be of a very delicate character and easily disturbed in one or the other direction.

Certain salts, especially the chromates, have been found to exercise a very marked protective effect and to prevent corrosion; this has been attributed to the iron being rendered "passive" by their action. It is by no means certain, however, that they do not act simply as salts of very weak acids—in other words, by neutralising the traces of acid present in the solution to which they are added.

The action of alkaline solutions on iron has already been referred to; mention should be made, however, of experiments which Friend has described on the action on iron, at room temperatures, of solutions containing both caustic potash and potassium chloride freely exposed to the air. His results appear to show that in the case of solutions containing small amounts of the salts, at first the corrosion is greater than in distilled water but that the corrosive action decreases as the amount of caustic potash is increased, becoming nil in the presence of 56 grammes per litre. It is remarkable that whereas in the absence of alkali corrosion proceeded uniformly, the metal being covered with a layer of brown rust which could be easily removed, when alkali was present the corrosion was local in character, deep pits being formed in places and the metal foil often eaten through. It would seem probable that electro-negative impurities in the iron play a special part in the latter case.

Effect of Corrosion on the residual Iron[1]

In his communication to the May meeting of the Iron and Steel Institute, Mr. Longmuir states the opinion, under the title "The Real Problem of Corrosion," that this "does not lie in the amount of metal lost by rust or decay but on the influence

[1] *The Corrosion of Metals.* By Percy Longmuir, B.Met.—*The Action of Aqueous Solutions of Single and Mixed Electrolytes upon Iron.* By J. Newton Friend and Joseph H. Brown.—*The Influence of Impurities on the Corrosion of Iron.* By John W. Cobb.—*The Corrosion of Iron and Steel.* By J. Newton Friend, Ph.D., D.Sc. (Longmans, Green & Co.). Price 6*s.* net.—*The Influence of Carbon and other Elements on the Corrosion of Steel.* Report of the Committee, consisting of Prof. J. O. Arnold (Chairman), Dr. W. E. S. Turner (Secretary), Prof. W. P. Wynne, Prof. A. McWilliam, Mr. C. Chappell and Mr. F. Hohson. (British Association Reports, Section B, 1911.)

exerted on the metal remaining intact and apparently unaffected. Deterioration is found which is not explained by mere loss in weight or by change in the ordinary composition of the metal."

He describes experiments in which eight-inch lengths of rail were cut up into sections and the pieces subjected to tensile and other tests. On testing pieces cut from certain used rails, in which cracks had been developed through defective track conditions, in comparison with those from unused rails, no sign of decay or deterioration could be observed in the faulty rails. Under other conditions, however, rails became seriously embrittled, this being the case especially in tunnel rails, particularly in those from damp tunnels. Whilst the whole of the results Mr. Longmuir obtained show the embrittling effect, no two samples were precisely alike; he was not able, in fact, to trace any regularity in the order of development of brittleness but considers that whilst erratic in intensity, it is certain in effect.

In many industries, especially that of wire drawing, cleaning with acid is resorted to in order to remove the scale from iron; the embrittling effect induced by this treatment is more or less recognised. Mr. Longmuir gives results obtained by himself showing the deleterious effect such treatment produces and the manner in which the original properties may be restored to the metal by heating it. The brittleness is attributed to the absorption of hydrogen.

Earlier in his paper Mr. Longmuir quotes analyses of rust obtained from rails laid in various situations, specially calling attention to the relatively large amount of sulphur present in them as sulphate, specially in that from tunnel rails; these, it is well known, are particularly subject to corrosion. It is clear, therefore, that the effect acids may produce should not be lost sight of.

The Influence of Impurities on Corrosion

As iron cannot be acted upon by an electrolyte except in the presence of an electro-negative "metallic" conductor, it follows that all conducting impurities which come within this category must influence corrosion; moreover, that alterations in the iron itself—whether in molecular or crystalline structure, which in any way affect the value either of E or of R in the

expression C = E/R—must also affect the rate and manner in which corrosion takes place.

This subject is dealt with by Friend in his book at considerable length but in a somewhat unsatisfactory and unconvincing manner. A useful paper on the subject is that read at the May meeting of the Iron and Steel Institute by Mr. Cobb. In this it is pointed out that iron is definitely electro-positive to most of its impurities; manganese and 80 per cent. ferro-manganese, however, are positive to iron. Mr. Cobb recognises that the impurity acts as the corrosion centre and that its influence depends more on quality and distribution than on quantity.

That common sense is beginning to prevail in such work is clear also from a passage such as the following:

"When a weak solution of picric acid is applied to an ordinary steel, the carbon eutectic is all etched out quickly, while the ferrite is unattacked. This is a misleading result, unless it is remembered that the characteristic pearlite structure of the carbon eutectic is that of alternating layers of iron and carbide of iron, which, when covered with such an electrolytically conducting film as dilute picric acid, become so many active electro-chemical couples. Thus the pure iron layers of the eutectic pass into solution, while the iron of the ferrite has no such opportunity, except along its surface contact with the eutectic. In a refined iron, which contains the eutectic in a very coarse form in a matrix of carbide, it can be seen plainly that the carbide lines in the eutectic itself, which penetrate from the matrix, are unaffected by etching."

Mr. Cobb then proceeds:

"From what has been said, it would follow that, if other conditions could be made exactly the same, the iron dissolved in a given time, on etching two irons containing no impurity but carbon, would be in the ratio of the amounts of the pearlite (eutectic) present. The all-pearlite iron containing 0·9 per cent. carbon would show the greatest rate of dissolution; any excess of iron, or of carbon, would lessen it."

In view of this pronouncement, the following statement is of interest in the Report on the influence of carbon and other elements on the corrosion of steel by Prof. Arnold and others presented to the Chemical Section at the recent meeting of the British Association. The Committee have examined a series of six iron-carbon alloys containing from 0·10 to 0·96 per cent. of carbon, prepared from Swedish bar iron and charcoal.

The determinations of corrodibility were carried out with samples subjected to heat treatment

"designed to resolve the pearlite into the principal varieties in which it usually exists in carbon steels—viz.: (*a*) Diffused, (*b*) Laminated and (*c*) Emulsified and also into Hardenite, which is the essential constituent of hardened carbon steels. The treatments employed were (1) As Rolled, (2) Normalised, (3) Annealed, (4, 5) Hardened and Tempered at (*a*) 400° C. and (*b*) 500° C., (6) Hardened."

The results indicate

"that the carbon exerts two types of influence upon the corrodibility, dependent upon the condition of the carbide in the steel. In the rolled, normalised and annealed specimens, in which the carbide (as shown by microscopic examination) exists entirely as the diffused, normal variety or as the laminated variety of pearlite, the corrodibility rises to a maximum at saturation point (0·89 per cent. carbon) and then decreases upon the appearance of cementite in the steel. The rise in corrodibility in such steels with increase of carbon from 0·10 per cent. to 0·89 per cent. is not regular but is much more rapid in the range 0·3 per cent. to 0·89 per cent. carbon than in the low carbon range 0·10 per cent. to 0·3 per cent. carbon.

"In the hardened and tempered specimens, however, in which the carbide has been converted respectively either to hardenite or the emulsified variety of pearlite, it has been found that the corrodibility rises continuously from 0·10 per cent. to 0·96 per cent. carbon, no maximum being observed at the saturation point. The proportional increase of corrodibility in these steels with rise of carbon per cent. is very rapid up to about 0·25 per cent. carbon in the case of the hardened steels and about 0·40 per cent. carbon in the tempered specimens. After these points the rate of increase of corrodibility with rise of carbon per cent. is small but regular up to 0·96 per cent. carbon, being in this respect the reverse of that found in the type first described.

"The state of division of the carbide in the pearlite is found to exert very considerable influence on the rate of corrosion of iron-carbon alloys. In general, the annealed steels in which the carbide exists entirely in the laminated condition show a minimum corrodibility, whilst the tempered steels containing the carbide as the emulsified variety show a maximum corrodibility—except in the very low carbon steels. The normalised steels, moreover, in which the carbide is in an intermediate state of division, being mainly of the diffused variety, take up an intermediate position. This indicates that the finer the state of division of the carbide in the pearlite, the greater is the liability to corrosion."

It is fairly obvious that the character and distribution of the electro-negative impurities must be the chief determining factors in corrosion : the sooner the problem is looked at and studied from this point of view, the better it will be.

Much is said in the Report referred to as to the "solution-pressures" of the steels. The results obtained with sea-water are said to be inconclusive (not sufficiently conclusive to warrant any definite statement).

It may be questioned whether, in such an inquiry, the conception be not one that should be set aside altogether as at most of purely academic interest, if it be not altogether misleading. Some account must be taken of the infinite complexity of the results obtained in dissolving materials which dissolve mainly in local circuits ; determinations of the electro-motive forces developed between such materials and a negative electrode cannot well be interpreted in any simple manner—this at least is the conclusion the writer came to years ago after spending much time in determining the electro-motive forces developed by copper alloys.

It is noteworthy and interesting that Mr. Cobb, who bases his opinion on his own observations, refers to manganous sulphide as a non-conductor of electricity which itself undergoes change but is without direct influence on the corrosion of iron. In the Report of the Commitee referred to, manganous sulphide is spoken of as being powerfully electro-positive and as tending to cause serious electrolytic action when present in steel, thus materially increasing thc corrodibility. The probability is that it acts indirectly because sulphuric acid is formed from it sooner or later by oxidation.

Mr. Cobb's experiments were made by exposing a small slab of iron in opposition to a slab of the material to be tested in the so-called feroxyl indicator which has played so considerable a part in recent discussions on the corrosion of iron. The indicator is made by neutralising a solution of gelatin—so strong that it will just set on cooling—with caustic soda and adding a little potassium ferricyanide and phenolphthalein. When such a solution is electrolysed and the iron is attacked, the region in which the attack takes place is indicated by the appearance of Prussian blue, whilst the region in which alkali is liberated at the electro-negative electrode becomes pink.

Mr. Cobb describes an experiment in which a slab of iron

and a similar cast slab of silicate of iron (ball furnace cinder) were put into a beaker containing the viscous indicator just about to set; they were connected externally by copper wire through a galvanometer. A strong current flowed immediately and the gelatin surrounding the iron began to turn blue. Next day, the slag had a large pink halo around it and the iron one of intense blue *topped by a thin ring of pink on the surface of the jelly.* Mr. Cobb attributes the production of the pink ring at the surface to a current flowing from one part to another of the iron bar, in consequence of the difference in potential existing between neighbouring portions owing to electrical inequalities in the bar; he does not fully realise apparently that the current which gives rise to the pink is conditioned initially and determined in direction by the oxygen polarisation of the bar at the surface of the liquid—though he is evidently alive to the fact that such polarisation is operative. This and other illustrations given by Mr. Cobb are, however, particularly interesting; in fact, his paper is one which better repays close study than any other I have met with on the subject.

After reading the various books and essays on the corrosion of iron, the impression that remains is that a very large amount of work has been done to little or no purpose, owing to the lack of guiding principles and the absence of clear thinking. On the practical side, little need be added beyond what was said in the earlier section of this essay: as there pointed out, it can scarcely be regarded as otherwise than established that rusting takes place only in presence of acid, although, paradoxical as it may seem, this condition is compatible with the manifestation of a certain degree of alkalinity, as in the case of weak solutions of alkaline carbonates exposed to the air.

Apparently, neutral salts have no direct effect in promoting rusting: certain salts, such as the chromates, have a distinct inhibiting effect but the manner in which protection is secured by their use is by no means clear. The ultimate conclusion is, without question, that, if it be desired to prevent iron from rusting, the access of liquid water together with that of acid and oxygen must be guarded against in every possible way: in other words, as acid is always present in our atmosphere, the iron must always be protected against moisture.

The Corrosion of Copper and of Copper Alloys[1]

The general lack of understanding of the conditions which determine the corrosion of copper and its alloys would seem to be even greater than is the case with iron. The chemical text-books give no sound information on the subject; even the fact that copper is dissolved by a hot solution of hydrogen chloride is very commonly overlooked, as Tilden pointed out to little purpose years ago (*Journal of the Society of Chemical Industry*, 1886, p. 84). Theoretically copper should not be soluble in chlorhydric acid if the interaction involve merely the formation of cupric chloride and hydrogen, as such a change would be attended with an absorption of energy. Thus, taking Julius Thomsen's figures:

CuO	= 37,160	gram-deg. C.	heat-units
CuO . 2HCl Aq	= 15,270	„	„
	52,430	„	„

since the heat of formation of water is included in this value, the heat of dissolution of copper in chlorhydric acid will be—

$$52{,}430 - 68{,}360 = -15{,}930 \text{ units.}$$

A like argument is applicable to nitric and sulphuric acids. Veley moreover has shown, by a most exhaustive series of experiments (*Roy. Soc. Phil. Trans.* 1891 A. **182**, 279), that nitric acid free from nitrous compounds will not dissolve copper; and it is well known that copper will not dissolve in dilute solutions of sulphuric acid or in organic acids, except in presence of oxygen.

The direct dissolution of copper by chlorhydric acid is probably to be accounted for as the consequence of the formation of cuprous chloride. The dissolution of copper by ordinary nitric acid doubtless takes place in a circuit which includes not only the acid but also nitrogen dioxide or some other nitrous compound which acts as the depolariser, throwing energy into the circuit. Hot sulphuric acid is a solvent of copper probably not because the acid acts when hot though

[1] "A Report to the Corrosion Committee of the Institute of Metals." By Guy D. Bengough, M.A., *The Journal of the Institute of Metals*, vol. v. No. 1, 1911.—*On the Failure of a Brazed Joint.* By Prof. Henry Louis.—*The Corrosion of Brass with Special Reference to Condenser Tubes.* By Paul T. Brühl, M.Sc., Meeting of Institute of Metals, Sept 1911.—*The Hard and Soft States in Metals.* The Second May Lecture of the Institute of Metals, May 12, 1911, by G. T. Beilby, F.R.S.

not when it is cold but because when hot it contains sulphuric anhydride, which acts as the depolariser—

$$Cu + SO_4H_2 + OSO_2 = CuSO_4 + H_2O + SO_2.$$

The fact that copper is soluble even in very weak acid solutions in presence of oxygen has long been obvious to all users of copper vessels. In every well-conducted kitchen, copper vessels are always kept scrupulously clean and dry; no properly trained cook ever allows food materials to stand any length of time in a copper pan. In recent years, from motives of cleanliness, brewers have been led to line their fermenting vats with copper and have been somewhat surprised to find that fermentations took place in a less satisfactory manner than in wooden vessels and that the growth of the yeast was appreciably diminished. The change has been traced to the presence of small quantities of copper in solution. Apparently, action takes place at the air-water line, the copper being dissolved in a circuit formed between the metal, atmospheric oxygen and the minute proportion of acid present in the wort. Brewers are being led to use aluminium in place of copper now that it is recognised that the copper is liable to dissolve in the cold wort.

Copper can be used safely in the kitchen and in constructing the huge vessels used in breweries in boiling the wort before it is fermented, because the amount of oxygen in solution in water is so very much reduced by heating it.

Attention has already been called (p. 314) to another problem awaiting solution in the case of copper, namely, whether pure copper will dissolve in acid in presence of an effective depolariser. Copper, as ordinarily prepared, probably contains sufficient electro-negative impurity to condition its dissolution, if such be required.

A knowledge of the conditions which determine the corrosion of copper alloys, especially of the copper-zinc alloys, is of importance, owing to the large use that is made of these alloys in engineering practice.

Attention has been called to the subject of late, as it has been under discussion at meetings of the recently established Institute of Metals. A long Report to the Corrosion Committee

appointed by the Institute was presented at the annual general meeting in May last which is a summary of the state of knowledge or rather, it should be said, of ignorance, of the precise conditions determining corrosion. It is claimed that the Report is the most comprehensive monograph available on the subject.

A brief introduction is followed by a review twenty-four pages long of the more important technical papers and a short synopsis of the views of their authors. The conclusion the reporter arrived at is that modern knowledge of the corrosion of copper is in a somewhat confused and tangled state and that very few positive experimental results can be considered as thoroughly established. Believing this to be due to the fact that the mechanism of the solution of the metal or alloy has not been taken sufficiently into account, he proceeds to restate the whole case from the point of view of the ionic theory of solutions. Having this bee in his bonnet, he gives a detailed statement of the "theory" and wastes fourteen pages in idealistic elementary talk about ions, solution pressure and all the various shibboleths of the school; but this is insufficient to inform the ignorant and useless to those who are informed. Not a single practical conclusion is arrived at; those who have some common sense in their composition who read the section must feel that they are left in the "as you was" position, with a completely confused mind—well prepared therefore, let us hope, to jettison the whole Report and begin afresh.

In the penultimate section of the Report the author restates the problem and then gives his own views of the problems involved. In the final section a scheme of research is outlined but this is as nebulous as the earlier part of the Report.

Nothing could be less satisfactory, nothing could show more clearly the disgraceful condition of ignorance which prevails on subjects so important as that of the conditions which determine the dissolution of metals: this notwithstanding the continuous babble about ionic dissociation and its consequences that has distracted the attention of chemists during the past quarter of a century.

Whether there be dissociated ions or not in aqueous solutions matters little—this is a question for academic discussion. The problem before us is the determination of the conditions which must prevail if attack is to take place.

"It may safely be assumed that all metals and alloys, when plunged into dilute acids or into water, either pure or containing salts in solution, tend to pass into solution." This is the opening sentence in the fourth section of the Report referred to above. Nothing of the sort can be assumed: so far as we know at present, nothing happens in the absence of an electronegative element from the system; the evidence is overwhelming that water pure and simple is without action.

In the fifth section we are told that Carnelly has shown that copper is soluble to a minute extent even in distilled water if oxygen be present. The reporter remarks that Carnelly's result is unexpected but is supported indirectly by other experimental evidence; he is not sufficiently clear on the theoretical side to realise that attack must have been determined by the presence of traces of acid in the liquid.

In the case of the copper alloys, we need information on all sorts of fundamental points. What is brass? Is it—within certain limits of composition—a compound of copper and zinc or are the molecules of the metals still possessed of properties which are characteristic of them when alone? Or in what proportions are the metals present separate and combined? Micrographic research is teaching us much on these points but there is a tendency to exaggerate its relative importance. A new nomenclature is being introduced, as in the case of steel—to the confusion of the practical man: but we don't get to grips with the real problems. We need to look at the matter both from the chemical and from the morphological side—and in neither case superficially; before all things also we need to introduce a large element of common sense into the inquiry and to cast pretence aside.

Many years ago Ackworth and I showed that a brass containing about 70 per cent. of copper behaved as copper towards nitric acid—as if by association with the copper the zinc were transmuted into copper. It is well known that much heat is given out when the metals are alloyed, so that evidently the zinc parts with some of its intrinsic energy in the process and is reduced at least to the level of copper. Long ago also it was shown by Laurie that the electromotive force developed by zinc-copper alloys was only that given by copper up to the point at which zinc was present in a proportion beyond that required by the formula Cu_2Zn. Probably, therefore, it is only

in the case of alloys containing such an "excess" of zinc that we must contemplate separate attack on the zinc as possible. No doubt the electro-negative impurities in brass play an all-important part in localising the attack but we have yet to learn the manner in which changes in the mechanical and crystalline structure of alloys affect corrosion. An interesting illustration of the manner in which impurities act was brought under the notice of the Institute of Metals recently by Prof. H. Louis, who traces the failure of a brazed joint to the presence in the braze of a fine network of an alloy probably of tin with lead, the planes thus formed being planes of weakness liable to yield under strain, thus allowing liquids to penetrate into the mass of the metal.

A question of special importance was raised by the correspondence in the *Engineer* in the year 1907 on the behaviour of Muntz metal in Australian water. It was then stated that this alloy has undergone corrosion more rapidly of late years, since it has been customary to make it from electrolytic copper instead of from that prepared by the old poling process. It is difficult to associate the idea of protection with the presence of impurities, unless it be that they affect the mechanical crystalline structure of the alloy in an advantageous manner. It is not impossible that electrolytic copper may be richer in cuprous oxide than polled copper and it is conceivable that this oxide may play an important part in facilitating the corrosion of copper.

In too many cases probably the deterioration which brass condensers suffer in use is due to careless working, especially to neglect to cleanse them properly when they are put out of use. On this account the final conclusion in the recent communication made by Mr. Brühl to the Institute of Metals is probably that most worthy of attention—viz. "That the tubes should be flushed with clean water after use"! This communication, it may be added, carries the subject very little further. Mr. Brühl evidently stands on an elementary footing, as he is in no way clear even why the presence of acid should be essential to the occurrence of corrosion. And yet he treats learnedly of the nature of corrosion in the usual stilted ionic style; but the subject is merely a hot potato picked up by the way and fortunately as soon dropped. A perfect gem of information is provided in this section, however, by the representation of the influence of

a speck of electro-negative impurity on a brass surface by four arrows at right angles pointing from four P's to a central N, the diagram being followed by the statement:

"If the impurity be a conductor of electricity the brass will pass from the atomic to the ionic state by the displacement of the hydrogen ions and consequently a moat will form round the negative centre. If the impurity happen to be positive, it will pass into solution with the production of a pit."

After reading this, the marine engineer will surely feel that the last word has been said on the subject and that what he does not know of corrosion is not knowledge.

I have written thus strongly, as I feel that it is important that something should be done to stop the misappropriation of ions in the service of corrosion and to emphasise the need of clear thinking. I would plead also that some imagination, some breadth of outlook, may be imported into the inquiry—and, before all things, that a sense of logic should be cultivated by those who essay to take part in the discussion.

H. E. A.

REVIEW

The Changeful Earth. An introduction to the record of the rocks. By GRENVILLE A. J. COLE, F.G.S., Professor of Geology in the Royal College of Science for Ireland. [Pp. ix + 223.] (London: Macmillan & Co., Ltd., 1911.) Price 1*s.* 6*d.*

THIS is one of the series of "Readable Books in Natural Knowledge" which Messrs. Macmillan are issuing. "It is hoped," the publishers say in their note, "that the books will provide the reading matter urgently required in connection with the science work in schools and will appeal also to a wide circle of other readers." That readable books on scientific subjects are urgently required is beyond question; it may be doubted, however, if it be possible to cater at once for two classes so different as the young scholar and "other readers"—the standpoints of the two are apt to be so different.

As the author remarks in his opening sentence, the book deals with a great subject. He then adds—"We have no right to live on the earth without trying to learn something about it." There must be a fair proportion of mankind, therefore, who have no right to live. The book under notice should certainly help a few more to qualify for an earthly existence: being written in Prof. Cole's well-known easy style, it should appeal to a wide circle of readers but perhaps more to grown-ups than to boys at school. We are inclined to think that it tells too little of things geological and of geological method and relatively too much of men who have been pioneer workers in geology. And the geological points are not always made with sufficient clearness—as on p. 186, for example, where the subject is far too easily dismissed by reference to a diagram which the ordinary person would not understand, knowing nothing of the order of strata.

No doubt this subject is treated in accordance with the intention expressed in the publishers' note but we are a little inclined to question the doctrine laid down in the note—boys are kittle folk, they like to get at the point and hate any approach to sentimentalism. Prof. Cole goes out of his way too, we think, to use inconsequent tall talk at times—for example, on pp. 5, 9, and 20. And p. 5 is full of bad literary example for youth: "The more we know of this science, the more grateful we are to those who laid its foundations, in times when travel was far harder than it is to-day." Such punctuation is inexcusable. Again, "he found that the rain that falls and the wind that blows continually change its surface; that one part of the world is growing, while another wastes away," leaves us in doubt whether it be the wind that always blows or the earth that is changed continually. We cannot expect boys to write clear English when such examples are set before them in the books provided specially for their use.

When, however, such minor blemishes are removed in a new edition, the book should prove to be of great value to a large class of readers. But Prof. Cole must exercise a little more thought before giving reins to his fancy. For example, it is very well to quote a grand old man like William Smith as saying:

"Natural History should be the first object of every country gentleman; and if

it be not an insult to nature to pass unnoticed her various productions, which are far superior to the nicest workmanship, it will sometime be an insult to the understanding to be considered totally ignorant of these things." In his day such speculation was legitimate, but is the comment now justifiable which Prof. Cole makes on the passage of which this is the conclusion?—

"There are thousands of British citizens and country gentlemen and hundreds of members of Parliament for that matter, trained in our public schools and universities, who look upon the study of the earth as something that in no way concerns them and who often would regard it as 'an insult to their understanding' if they were supposed to know anything about it. They leave scientific studies, which they call 'stinks' at school, to those who are going to invent something for our material comfort, such as a new electric railway or a patent sanitary soap."

Is not an ever-increasing weight of evidence tending to show that scientific studies after all are a kind of mental caviare—that the vast majority will attend only to things of human interest? The late birth of science would seem to be a matter of more significance than is commonly supposed. It is a favourite educational doctrine that the development of the individual is more or less a copy of that of the race.

One remark in conclusion. The common practice is sufficiently objectionable which allows the signature of the photographer to be attached to each indifferent reproduction of an indifferent picture: an author should refrain from this practice. Prof. Cole might as well have attached his name to each page of his letterpress as to each of his pictures.

THE ETHER OF SPACE AND THE PRINCIPLE OF RELATIVITY

By SIR OLIVER LODGE, F.R.S.

Is space filled with a continuous medium or is it occupied only by segregated masses of matter?

Does continuity reign supreme or is discontinuity the fundamental fact of the physical universe?

These and like questions, in many different forms, have been asked throughout human history, yet no fixed and universally accepted answers have been given.

To Newton, to Huyghens, to Young—yes, to Faraday also and Clerk Maxwell—the existence of a universally connecting continuous medium was more than a working hypothesis; it seemed to be a necessary conception ingrained in the texture of their minds. To them pre-eminently, physical action at a distance, in the technical sense, was unthinkable.

But it has not been so with all great minds. To some—to Lord Kelvin for instance, not to mention the living—it appeared to be merely a question of evidence; the fact of inconceivability, if it were a fact, was not regarded as an invincible barrier to the truth of a given proposition.

To these physicists, apparently, cohesion seemed a variety of gravitation and gravitation might extend across empty space. Atoms need not be joined up by any intervening medium; there may be absolute void between them and yet they might still gravitate and cohere.

But how can they radiate? how can the quiver of one excite corresponding tremor in another? By bombardment of corpuscles it could be done—by some form of return to the corpuscular theory of light. Periodic, truly, such bombardment must be; "fits of easy reflection and transmission" are wanted, for periodic in space and time radiation certainly is; but the term "wave" may be too pictorial—it is easy to write down equations for undulatory light without conceiving any continuous etherial medium in which the waves exist.

For they are not mechanical waves, they are electromagnetic; who is to say what the actual mechanism of such waves really is? Why try to mix up ideas appropriate to the elastic solid theory of light with more modern conceptions?

Moreover, every attempt to bring the ether to book, to force it out of the realm of conception into that of demonstrable physical fact, to exhibit its presence and function by direct experiment, has hitherto been found impossible; experiments directed to that end have always yielded a negative result. True there is astronomical aberration but the interpretation of aberration is simplest on the corpuscular theory; ideas connected with an ether appear only to complicate it—just as the propagation of light in straight lines or rays is far simpler on a corpuscular theory. But then *a* wave theory is true; and waves have seemed to demand a medium in which those waves exist—such a medium cannot possibly be ordinary matter; moreover, an ether of finite properties has seemed to be demanded for the known very definite velocity of wave propagation.

But is velocity as definite and unalterable as we had imagined or is its apparent constancy to be otherwise explained when moving bodies are taken into account? So there arises the question how the transmission of light is affected by motion, motion of source relative to medium—if there be one—as well as the apparent effect due to motion of receiver relative to source.

Innumerable attempts have been made to detect some result of ether drift past the earth—some result, that is, due to the motion of source and receiver together through a stationary wave-conveying medium—some influence on frequency or wave-length or direction or intensity due to the motion of the earth through an ether: and all fail.

One positive result there is, one case of speed of light being affected—but then this occurs only when matter is artificially moved—the great result of Fizeau.

But that is no case of absolute motion; matter is being moved relatively to the earth. The Fizeau effect concerns the change in the velocity of light produced by a stream of water up or down which the light is travelling; and no wonder the result is positive. It demonstrates that the motion of matter affects the speed of light inside that matter but it does not tell us by what mechanism that effect is produced. Something of

the kind would be expected on almost any theory. The refractive index properties—the μK properties—of matter must be expected to travel with it.

But as to the carrying of ether along *near* matter by a kind of viscous or mechanical connection, experiment shows that close to rapidly moving steel there is no perceptible effect on the velocity of light, so that the ether appears to be absolutely stationary. But then another experiment yields what at first sight appears to be a precisely contrary result—namely, that light takes precisely the same time to travel to and fro whether its path lies across or along the direction of the earth's motion. For this is just what it would do, if the ether near the earth were clinging to and carried with the earth ; just what it would not do, if the earth were moving through a stationary medium—a medium totally unaffected by its motion. And in general we may say that in all experiments no drift of stationary ether past the moving earth has ever been detected ; thus arises the suggestion that in the neighbourhood of the earth the ether is carried along so as to be relatively stagnant with reference to the earth.

If a single effect due to the motion of the earth through space could be detected, we might have some hope of demonstrating a sort of absolute motion, *i.e.* a motion of the planet with reference to the universal and presumably stationary infinite connecting medium.

Failing that discovery, what must be our conclusion ? There are some who think that this must be our conclusion :—That absolute motion of this kind can never be detected, that there is no sense in it, that no universal stationary medium exists, that humanity can never detect anything but the effect of relative motion of one piece of matter with respect to another. Relative motion is familiar enough ; absolute motion—or motion relative to an imaginary ether of space—is either meaningless or for ever beyond us. The Principle of Relativity is dominant.

So—though in other words—runs the argument ; and if the principle of relativity is ever really established, it will give a death-blow to the Ether.

The principle of relativity has to-day attracted the favourable attention of many great minds. To the writer it appears a confusing mistake and he wishes to contend against it to the utmost.

Can the issue be raised in a simple concrete fashion and the distinction between the two clearly formulated in a single instance?

Yes: consider the radiation from a uniformly moving body through uniform space, all receivers and modes of detecting the light moving with it. On a projectile theory and likewise on the principle of relativity, successive wave-fronts—if the name be still permissible for disturbances originating together and still in the same phase—will be and will remain concentric spheres. For on the corpuscular view those emanating in the van will have the body's own speed super-added to their ejection velocity, whilst those flying to the rear will fly with less speed, so that their distances from the source of those simultaneously ejected will remain in every direction the same.

On the relativity view the question will not arise, for the supposed motion through mere emptiness is absolute motion and therefore undetectable or meaningless. The unit of time may be defined as the time taken for light to travel unit distance from the source in any direction; there is no way of detecting such motion of a relatively stationary system of bodies as has been supposed. Pragmatically it is non-existent.

On the ether theory, however, occurrences will be quite different. The waves are now waves in a stationary medium, having a definite velocity of propagation depending on the properties of that medium and on nothing else, not at all dependent on what the source may be doing, not in the least accelerated or retarded by any motion of the source. The wave-fronts are like similar swimmers setting out in regular succession, in all directions, from a slowly moving ship in calm water; they will be closer together in the van than in the rear but all their speeds will be identical. The corresponding set of swimmers will remain on circles but they will not be concentric circles, the ship will be steadily moving away from each centre of emanation. So also the waves relatively to the source will be subject to uniform drift backwards; their section will be a system of coaxial circles.

Yes, opponents will say, we know well enough that that is a consequence of the ether theory but how do you prove that this occurs, how can you verify the conclusion that things really happen so for a source fixed to the earth? How can you, travelling with the earth, employ any instru-

ment to demonstrate the excentricity of successively emitted waves? Did not Michelson and Morley try, with consummate skill and beautiful apparatus? did they not show that there was no effect to be observed?

And we reply, yes truly that is so, we do not contest the fact nor seek to evade the definiteness of their result. But then, we go on to say, FitzGerald and Lorenz with equal definiteness showed that on an electric theory of matter all such effect ought to be null, precisely as observed; they showed that moving apparatus would be subject to distortion, its length and breadth would not be the same while moving through the ether as they were while stationary—the force of cohesion, if electrical, must be changed by motion—not by relative motion only but by absolute motion through the ether. Since Clerk Maxwell, it is surely admitted that two moving charges repel or attract each other less than they do when stationary; and hence, even quantitatively, the effect that Michelson and Morley were looking for ought not to be discoverable, unless they could conquer the inevitable distortion of their block of stone.

Mere subterfuge! we must suppose our critics to reply, a transparent device for evading a straightforward and simple conclusion. The effect is not detectable, they would continue, simply and solely because it does not exist. It is unscientific or worse to seek to get over a distinct answer of experiment by inventing some ingenious compensatory cause which previously you had not thought of.

True we had not thought of it before but we might have done; it is a direct consequence of our theory of matter. And the negative result of the Michelson and Morley experiment, so far from throwing doubt on the existence of an ether, pleases us by confirming our deduction of the drift effect which does occur and would be detected but for the compensating cause; likewise by verifying what we now perceive is a genuine though unexpected result of motion—the minute alteration of shape, the pure shear, which moving matter undergoes—a result which we should have been able to detect and verify in no other way.

"Are you then satisfied," they might ask, "to base two deductions on one experiment with a clear negative result, on the ground that both are reasonable and necessary and that the effect of either alone would have been perceived although the effect of the two together is demonstrably null and is proved to

be so by experiment?" To which we reply, "No, we are not quite satisfied, the proof by a null method cannot be considered finally satisfactory but it is the best we have attained so far; at any rate it is better than your juggling with *Time* and defining the *second* to suit yourselves, so as to contrive artificially not only that you cannot measure any effect due to motion but that there can never be any effect to observe."

The change of shape caused by motion is at least a physical result, predicted at present, subsequently to be verified. Whereas the definition of a unit of time, in such a way as forcibly to neutralise differences of speed in different directions—even though the stationary medium is really drifting relatively past a moving body—is not based on a discovery or suspicion of a compensatory influence but is a tampering with fundamental natural entities in an illegitimate and preposterous manner.

Time and Space are separate entities; the course of one is not to be disturbed by supposed exigencies in the other. The excuse no doubt is that they are both abstractions derived from our direct sense of motion and speed. But abstractions though they may be, they are either unreal or they are independent and absolute—something over which we have no control, into the conception of which no mere physical mechanism can enter.

If clocks go at a different rate according to the way they are moving—as they may, by a shortening of the pendulum—that is a definite assertion which may be stated and some day may be tested. But the varying rate of a clock has no influence whatever on the Course of Time. Time is one thing, its measurement is another; the bad going of a clock is an affair of mechanics, not of metaphysics nor philosophy.

Nor in our judgment has the motion of a source any influence whatever on the speed of light. Light travels through the ether, not through matter; whatever matter does, ether is stationary. The only effect of moving matter is that which corresponds to its known refraction and dispersion effects and into these motion enters in a known way.

"How then!" Opponents will say, "Are you going to uphold the intelligibility of truly absolute motion—motion with respect to no axes of reference or to anything whatever—motion in the abstract, through the void?"

"No," we reply, "not so but motion through the ether. That

is what we mean by absolute motion, as distinguished from relative motion with reference to masses of matter. Such relative motion is not all that we can ever know. Motion with respect to a universal medium is not meaningless and is not as undiscoverable as you imagine. It has not been discovered but we have by no means abandoned the attempt.

Besides, surely you admit *some* meaning in absolute motion beyond your present arbitrary definition! For what did Copernicus and Galileo contend? Does it after all matter nothing whether the earth is spinning on its axis or whether the sun, moon and planets and all the hosts of heaven are revolving round it in twenty-four hours? On the principle of relativity it makes no difference, no practical difference, nothing that can be detected by experiment. So long as their relative motions are all that can be perceived, it will be said, it is plain that of their absolute motions we know and can know nothing. Whether Arcturus revolves round the earth or whether the earth rotates is merely a matter of convenience of expression.

If there be an Ether, we grant you that Arcturus could not have such a speed as would be needed; but in absolute void, what is to prevent it from going at an infinite speed—or rather what meaning can be attached to speed at all in such a case?

What have we to oppose to such a contention, beyond popular prejudice and scientific or perhaps unscientific instinct?

Well, we might urge the theory of electromagnetism. A charge in motion is an electric current. The earth is charged so that if in motion its line of advance is surrounded by magnetic lines of force and essentially every electron on it is also a linear current. With the speed of the earth estimated as twenty or thirty miles a second the current is insignificantly weak. Currents due to charged bodies moving with the earth have never been observed. But if the earth had a speed such as that we just now postulated as thinkable for Arcturus or let us say for the famous *Nova* of 1901, what kind of current would it equal then? Three hundred light years multiplied by 2π and divided by a day gives a speed 1800×365 times that of light or 2×10^{16} centimetres per second. If the earth had a speed like that, a charged pith ball might be a current of a million amperes.

The anticipated reply is that we should never know. We should be moving with it and no effect due to its motion could

be detected? A gratuitous statement this, not a likely one; but for the present it must pass. The electromagnetic influence of two bodies moving side by side has not yet been detected except by means of apparatus stationary in respect of both bodies—thus introducing relative motion. Whether the plates of a charged condenser fixed to each other and to the earth—*i.e.* moving only with the earth—do really attract each other differently or have a different capacity or anything of the kind by reason of that motion remains a point not yet settled by observation.

So, naturally, the supporters of the doctrine of relativity have a great deal to say in support of their position; certainly they can contend that no experiment has yet assailed it.

The weak point of their position—and fortunate it is that it has this weak point, for thereby it cannot remain as a permanent bugbear in the path of science unless it has a real and substantial justification, in which case it will be no bugbear but the truth—the weak point is that any year an experiment may be made with a positive result; unlikely though such a thing seems at present, by reason of curiously compensating causes.

Whenever that happens, the bubble will be pricked; and physicists in general—not the loyal few alone but the adventurous spirits who have struck out in rash metaphysical directions of their own—will return to their allegiance and once more interpret physical nature in terms of an omnipresent, universally-connecting, perfectly-continuous medium, the Ether of Space.

THE STRUCTURE OF THE ATMOSPHERE AND THE TEXTURE OF AIR CURRENTS IN RELATION TO THE PROBLEMS OF AVIATION[1]

By W. N. SHAW, F.R.S., Sc.D.
Director of the Meteorological Office

In this essay it is proposed to bring under notice some of the recent advances in our knowledge of air currents made in this country and to consider their application to the problem of navigating the air. I shall deal mainly with recent work by Mr. Cave of Ditcham Park, Mr. W. H. Dines and his son Mr. J. S. Dines at Pyrton Hill, Mr. Salmon at Brighton, the Meteorological Department of the University of Manchester and my colleagues of the Meteorological Office, Mr. Lempfert and Mr. Corless.

I suppose that when the first sailor put to sea in a boat he must have thought over the phenomena of waves, tides and currents so far as they could be made out from the shore. His experience was very likely somewhat different from his most intelligent anticipations. It is doubtful whether any amount of study from the dry land would have enabled the primitive navigator to deal successfully with the dangers of the deep. The art of navigation must be learned afloat; but a little experience combined with sufficient observations from the shore might suggest some of the situations in which discretion is the better part of valour.

So it is with the airman and the atmosphere. No meteorologist can teach an airman how to fly. Whatever may be our knowledge of meteorology, the art of aerial navigation must be learnt in the air; but we can tell the airman something of the details of the structure of the atmosphere which is to support

[1] The substance of two lectures delivered at the Royal Institution on May 18 and 25, 1911. References to a bibliography at the end are given by numbers in brackets.

his machine and put him in possession of information which will enable him to distinguish between the situations that are manageable with skill and those that are not. The first airship that had to face a line-squall was wrecked. The proper advice to an airship about a line-squall is to avoid it.

It is therefore useful for those who are interested in flying or floating to know what are the atmospheric analogies of the tide, the current, the wave, the whirlpool and the bore. The knowledge is difficult to attain because one cannot see the variations of level and of position of the moving air as one can those of the surface of the water; moreover, it is not merely the surface layer that we have to deal with. Air craft may be navigated at any height up to, say, 10,000 ft. We are only just beginning to know what goes on in the atmosphere within the lowest two miles.

I propose to arrange what we have found out about the atmosphere that may be of assistance to airmen in their difficult enterprise under the headings "The Structure of the Atmosphere" and "The Texture of Air Currents." Under the first heading I will deal with the different currents that may be met with at different heights; the modifications of the currents of the lower strata up to, say, 1,500 ft. attributable to the general effect of the friction of the ground; and the local effects of obstacles of sufficient dimensions to cause turbulent motion in the lowest layer. If we picture to ourselves a great current of water flowing over a rocky bed, we may visualise a bottom layer that has to find its tortuous way round obstacles of various sizes and shapes; above that a stratum where the direct effect of the bottom becomes gradually less and less marked; ultimately, the remainder of the 10,000 ft. of thickness, where the effect of the bottom is lost or only appreciable by comparatively slow and regular changes. When we get to these elevations, we must be prepared to find strata which move under general conditions different from those of the lower layers. We may have to deal with currents different in direction as well as in speed, because the forces which control the motion are different.

There are currents of the atmosphere, analogous in many ways to the currents of the ocean or the river but different altogether from them in the speeds which may be reached. We may expect air currents, high up, to reach a speed of more than 100 miles an hour, a speed which in water would be indeed a serious obstacle to successful navigation.

I shall call attention to the want of homogeneity of air currents, to the manner in which a broad current seems to be formed by streams of different kinds of air flowing alongside, out of which there appear to be developed, under suitable conditions, the phenomena of the line-squall. These are the bores and perhaps the whirlpools of the atmosphere.

I propose to say a good deal about the line-squall, because it is characteristic of the most vindictive and most destructive phenomena of weather. It would appear that in Greek mythology the line-squall is personified in the Harpies, "the Snatchers." I shall have to describe some of the characteristic phenomena of the line-squall before I can make good this suggestion but I notice that there is a tendency in these days to revert to Greek mythology for appropriate meteorological expression. If "anticyclone" is to be displaced by the more poetic name "halcyon," that has its associations with the kingfisher, let me suggest that the airman should learn to look upon a line-squall as a harpy and deal with it accordingly.

Under the second heading, "The Texture of Air Currents," I shall consider the variations in a homogeneous current and show that in the atmosphere what passes for a steady blow is not by any means free from variations in direction and speed. I shall show what we have been able to make out of the details of these variations. They are analogues of the waves of the sea as regards the comparative rapidity of the variations but we shall see that they cannot be called waves in any dynamical sense.

Structure

In dealing with the structure of the atmosphere I shall take first the phenomena of the layer within which the friction of the surface is still operative and the relation of that layer to the upper undisturbed currents. In dealing with this part of our subject, when we consider the changes which take place, we require a standard to which the variations upon any occasion can be referred. One might naturally suppose that the surface wind would be the proper standard; for the purposes which I have in view, it is not at all a convenient standard because the measure of the surface wind depends so largely upon the exposure of the particular anemometer which measures it. The surface wind at two places not many miles or even many yards

apart may be different, not because the meteorological conditions are different but because the instruments are differently affected by local obstacles to the motion of the air. I propose to take the gradient velocity as the standard and will endeavour to explain what I mean thereby.

If we take an ordinary weather map upon which the distribution of pressure is represented by isobaric lines (see for example fig. 9) we find that the observed wind at the coast stations is more or less along the isobars and is strong when the isobars are close together, feeble when they are far apart.

Let me ask: What kind of motion is this wind motion? It is clear that it is closely related in some way to the pressure. It must be borne in mind that there are two kinds of motion, both of which may be illustrated by a pendulum bob: first the motion which is displayed when the bob is pulled aside and let go, the other when it is projected so as to describe a circle. In the one, the motion is along the line of the force and the velocity increases rapidly as the bob gets towards the centre; in the other, there is no increase of velocity, only change of direction, because the force is perpendicular to the direction of motion. With the winds the forces are the pressure differences and very little increase or decrease of velocity is produced in seconds, minutes or even hours (1). Which of the two kinds of motion best represents the wind? The forces are not quite perpendicular to the motion but nearly so; the persistence of the velocity, with some other evidence which I will not give here, is proof enough that it is the motion with the balance of force by change of direction that is to be taken. The rotation which has to balance the force is not pendular rotation but the rotation of the earth; if we can assume that the rotation of the earth just balances the pressure-gradient, we can calculate the relation between pressure and wind for air which moves along a great circle of the earth by the formula (2)

$$\gamma = 2\,\omega\rho\,V \sin\lambda \quad \text{or} \quad V = \frac{\gamma}{2\,\omega\rho \sin\lambda}.$$

We may call the velocity thus calculated from the pressure the *gradient velocity* and the wind which has the gradient velocity and is along the isobar the *gradient wind*. We will use that for our standard of reference. It is evidently not dependent upon local exposure; for that reason, it is a better standard than the local surface wind.

A diagram (10) representing actual winds in relation to gradient winds will make clear the advantage of the gradient wind as a standard of reference. A circle (fig. 1) is taken to represent by its radii the velocity of the gradient wind for any point of the compass for the neighbourhood of Falmouth. Corresponding thereto is represented also, by radii for the several directions, the velocity of the actual wind as recorded at the Falmouth Observatory and the actual velocity given by an anemometer at Pendennis Castle, about two miles away. The differences between the two records are due simply to exposure; it will be seen that they are very large. The Pendennis exposure is much better than the Observatory exposure, that is

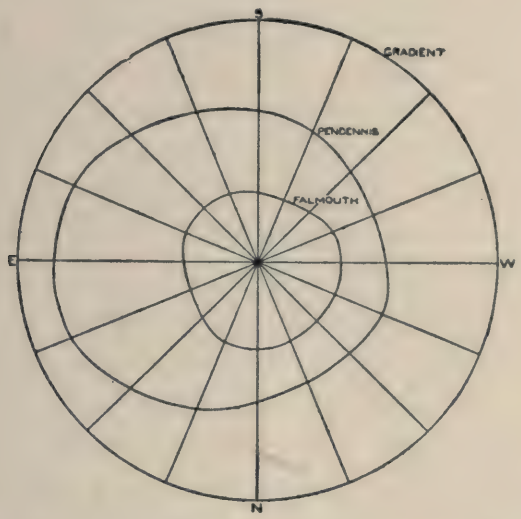


FIG. 1.—Velocity of the surface winds at Falmouth and Pendennis Castle in relation to the velocity of the gradient wind.

to say it gives a closer approximation to the gradient wind but it is not perfect; for westerly winds, the observed velocity is only about half the gradient velocity.

The determination of the barometric gradient, from which the gradient velocity is computed, requires a map or at least readings of the barometer at three points of a triangle surrounding the station. For the calculation of the gradient wind a table of wind velocities corresponding with distances apart of isobars on the map in nautical miles is the most convenient plan. Such a table is given in a Memorandum on Wind Structure published in the Report of the Advisory Committee for Aeronautics, 1910 (3).

Since measurements at several stations are required, the

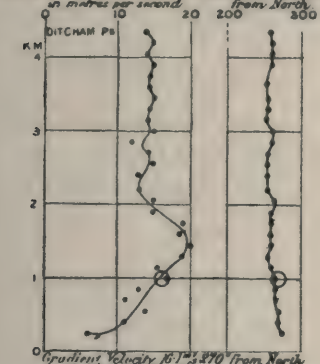
gradient-wind cannot be determined immediately at a single station; it is nevertheless desirable to keep the idea as the basis of a standard of reference for wind measurement.

Let us next consider the general structure of the atmosphere as disclosed by observations of pilot balloons. The process of observation consists in noting from minute to minute the position (altitude and azimuth) of a small balloon, inflated with hydrogen, by means of a theodolite or two theodolites and computing the vertical and horizontal distances traversed in each minute. We cannot at present make any general statements as to the structure of the upper air from observations of this kind; we can only give some examples and say that the cases which have been investigated may be grouped in certain types. In the diagrams (3) of fig. 2 the wind direction and the wind velocity are represented separately. The gradient velocity is marked on each of the diagrams. On examination it will be noticed that all types have a common characteristic, namely, that the wind velocity increases from the surface and approaches the gradient velocity with a veering of the direction of the wind through about two points. The height at which the gradient velocity is reached is not uniform but, generally speaking, a near approach to it is made by the time the level of half a kilometre is reached. Therefore we may divide the upper air into two sections, the first of which reaches from the surface up to the level of the gradient velocity of surface isobars, the second extending from the lowest level of the gradient velocity upwards.

We have a great number of observations of the velocity of the winds in the first 3,000 ft. from soundings of the air by means of kites as well as by pilot balloons (4). The results on different occasions are somewhat irregular but one conclusion may be briefly stated, namely, that the change of velocity with height is more fairly represented by increases proportional to the surface velocity than by the addition of fixed increases depending on the height and not depending upon the surface velocity.

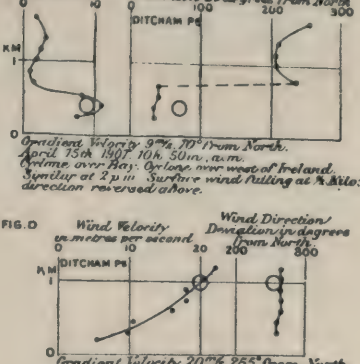
We get the upper wind from the surface wind by multiplying by a factor, not by successive additions of a fixed sum. From this generalisation we have the glimmering of a law that the velocity of the wind is proportional to the surface velocity and to the height above a datum level. The datum level is not perhaps really the same for all ascents, all wind directions and

FIG. A



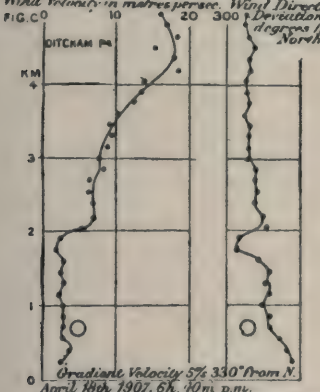
Gradient Velocity 6.1 m/s 270° from North
April 9th 1907. 6h. 18m. p.m.
Regular increase to gradient velocity, afterwards fairly steady.

FIG. B



Gradient Velocity 9 m/s 310° from North.
April 15th 1907. 10h. 50m. a.m.
Cirrus over Bay: Cyclone over west of Ireland.
Similar at 2 p.m. Surface wind sailing at 4 Kilo: direction reversed above.

FIG. C

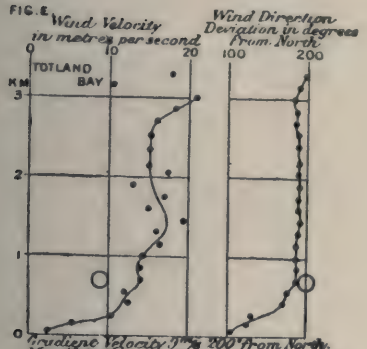


Gradient Velocity 5 m/s 330° from N.
April 18th 1907. 6h. 40m. p.m.
Central part of Anticyclone over station. steady light air.

FIG. D

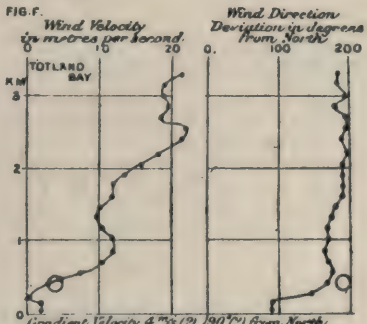
Gradient Velocity 10 m/s 255° from North.
May 3rd, 1907. 2h. 33m. p.m.
Steady direction, regular increase of velocity

FIG. E



Gradient Velocity 9 m/s 200° from North.
May 10th 1907. 6h. 32m. p.m.
Cyclone off West coasts increasing from surface to gradient steady in force and direction. Surface wind is down the gradient, not along the isobars.

FIG. F

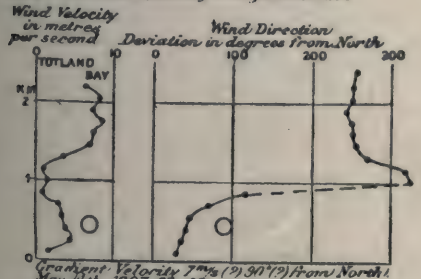


Gradient Velocity 6 m/s (?) 190° (?) from North.
May 17th 1907. 5h. 40m. p.m.
Cyclone off west coast. local squall over Irish Sea causing irregular distribution
Irregularity of wind at surface gradient velocity at 500 metres.

VARIATION OF WIND WITH HEIGHT
DITCHAM PARK AND TOTLAND BAY
FROM OBSERVATIONS OF PILOT BALLOONS
BY MEANS OF A SINGLE THEODOLITE
BY C.J.P. CAVE

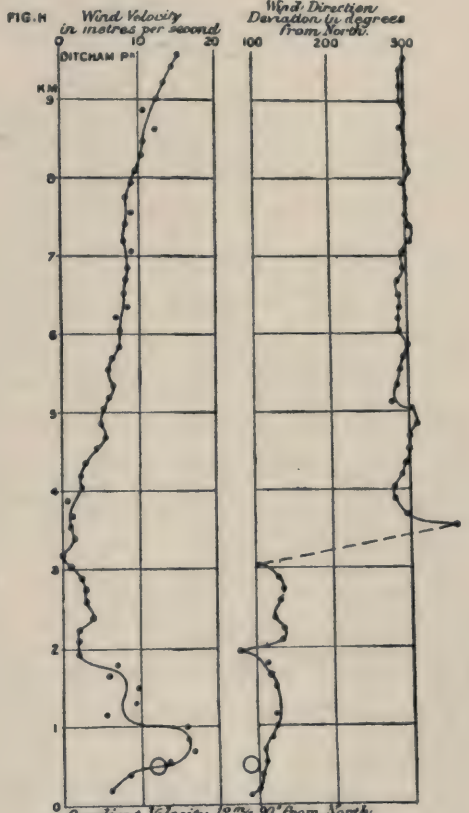
FIG. G.

The circles represent the Velocity and direction respectively of the gradient wind



Gradient Velocity 7 m/s (?) 30° (?) from North.
May, 14th, 1907, 6h. 53m. p.m.
Cyclone over Channel and France: cyclone also over Shetlands stretching to east coast of England. Surface wind not related to isobars.

FIG. H



Gradient Velocity 12 m/s 30° from North.
Nov. 6th. 1908. 10h. 39m. a.m.
The above gradient velocity is that due to the local distribution. the general distribution of pressure gives a velocity of about four metres per second. Stormy surface wind, increasing to gale force, falling off completely above 1 kilometre.

FIG. 2.

all stations: for a rough approximation we may take the datum level as sea level and take the velocity at any point above our heads to be proportional to the surface wind and to the height of the point above sea level, so that the facts may be roughly represented by fig. 3.

Ox represents sea level, points along Og heights above sea level; h is the height of the surface anemometer above the sea level, H the height at which we want to find the velocity. Take hv to represent the observed wind and draw HV parallel to Ox. Join Ov and produce it until it meets HV in V. Then HV is the effective velocity of the wind at the height H.

The increase of wind thus described will go on until the gradient velocity is reached. Take OG to represent the gradient

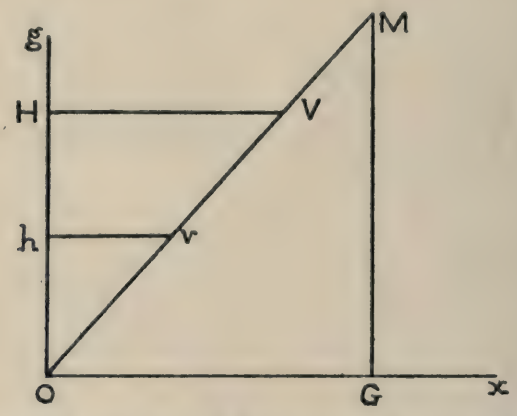


FIG. 3.—Variation of wind with height above sea level in the surface layer.

velocity. Set out GM, a vertical in the diagram. Produce OvV to meet GM in M. Then the proportionate increase will go on until the point M is reached.

This is a rough-and-ready representation of the phenomena of the surface layer. I am aware that objections can be raised to it on practical and theoretical grounds but I will not now discuss the evidence for its practical effectiveness nor attempt a better theoretical curve than the straight line to represent the transition from the surface wind to the gradient wind; I would recommend aeronauts to use this rough approximation and see how it works. For many purposes it is sufficiently near the truth. It should, however, be remarked that the regular increase of velocity up to the gradient-wind is deduced from observations made by means of kites and pilot balloons. The observations all belong to the daytime and the inference is only

applicable in practice for the daytime. At night time we have a new set of conditions, because the wind is subject to a marked diurnal variation. Unless the meteorological conditions are undergoing large changes the wind falls off at sundown and only begins to rise again after sunrise.

The following figures for the average wind velocity during July and August as recorded at different observatories in connection with the Meteorological Office illustrate this general statement:

TABLE OF AVERAGE WIND VELOCITIES IN JULY AND AUGUST

Observatory.	Average Minimum about 4 a.m.	Average for 7 a.m.	Average Maximum about 2 p.m.
	Miles per hour.	Miles per hour.	Miles per hour.
Kew	5	6	9
North Shields . .	8	9	11
Glasgow . . .	6	6	9
Stonyhurst . . .	5	6	9
Plymouth . . .	7	8	13
Brighton . . .	9	9	12

The difference between the maximum and minimum is most marked at the more inland stations. It is to be noticed also that the percentage differences between the maxima for the different places are less than those for the minima.

The diurnal variation of wind and its variation with height are effectively shown by the two diagrams of fig. 4, the first of which shows the records of an anemometer on the roof of the Bureau Central Météorologique, Paris, the second those of an anemometer at the top of the Eiffel Tower, 1,000 ft. high and close to the Bureau. The diagrams give the average values of the wind velocity for each hour of the day in each month of the year; the lines which are drawn connecting the figures of equal value show that at the foot the highest average velocity of something over 3 metres per second is in the middle of the day in January, February and March, the minimum being in the night with an extreme of only 1 metre per second at 5 a.m. in September. At the top of the Tower the minimum is from 8 to 10 a.m. in June but it is not less than twice the maximum at the low level, whilst the high level maximum is as much as 11 metres per second at midnight in January.

On account of the diurnal variation, the evening and early

morning hours are specially suitable for flying, because they make starting and landing easy and these are the most difficult periods of a flight. The explanation usually given of the meteorological fact that the wind falls off at night, which is quite well known, is that during the day the lowest layers of the atmosphere are constantly churned up by the effect of the sun upon the surface of the ground and that at night, on the contrary, the cooled surface air can remain on the ground undisturbed by convection currents. Thus the effect of obstacles on the ground and surface friction is much more marked. The surface layer may be actually at rest while the motion goes on

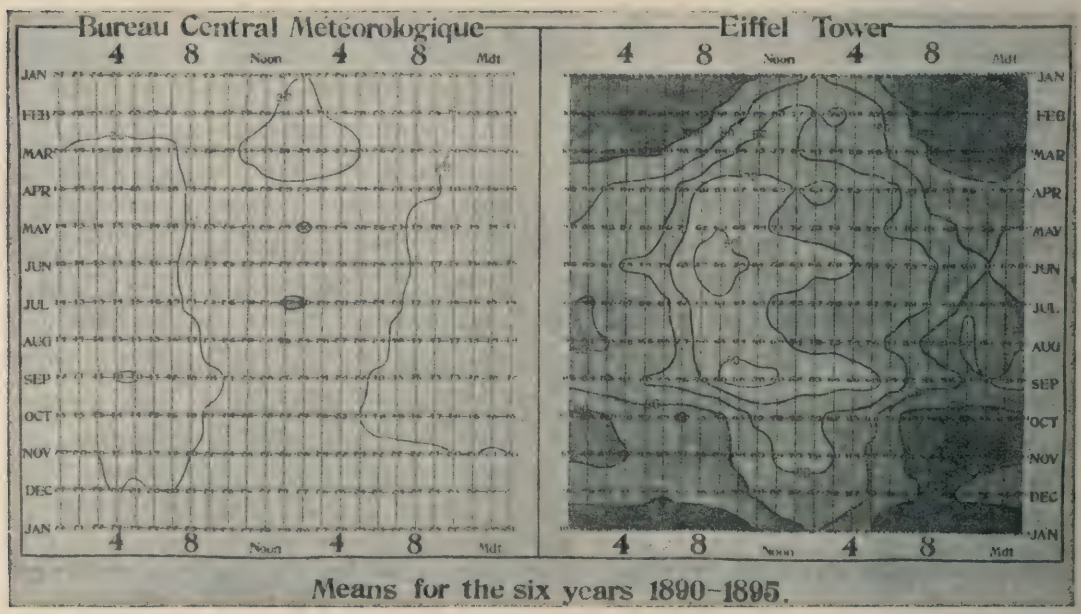


FIG. 4.—Isopleths of wind velocity for different hours of the day in different months of the year at the Bureau Central Météorologique, Paris, and at the top of the Eiffel Tower (1,000 ft. above ground). The isopleths are drawn for steps of a metre per second. The lines on the left-hand diagram are for 1, 2 and 3 metres per second, those on the right-hand diagram for 6 metres per second and successive steps up to 11 metres per second.

overhead even more vigorously than in the daytime, because the influence of the surface does not reach so far as in the day. Presumably the night curve of variation with height is a modification of the straight line to a curve something of the form represented in fig. 5; we have no observations from which to deduce its actual shape.

Balloons and airships manœuvre mostly within 3,000 ft. and it is the gradient wind with which they have to deal except in starting and alighting. In computing the path of a free run the gradient gives better information than the anemometer.

Beyond the layer of the gradient velocity many things may happen and for the present we can only classify the observed instances into types (5). Let me enumerate some of them:

1. The gradient velocity having been reached the wind may be steady in direction and velocity as we go aloft—we get a "solid" current.

2. The gradient velocity may be surpassed and the velocity in the higher levels become two or three times as great as the gradient velocity.

3. The direction may change continuously without much change of speed.

4. The speed may fall off nearly to zero and above a layer

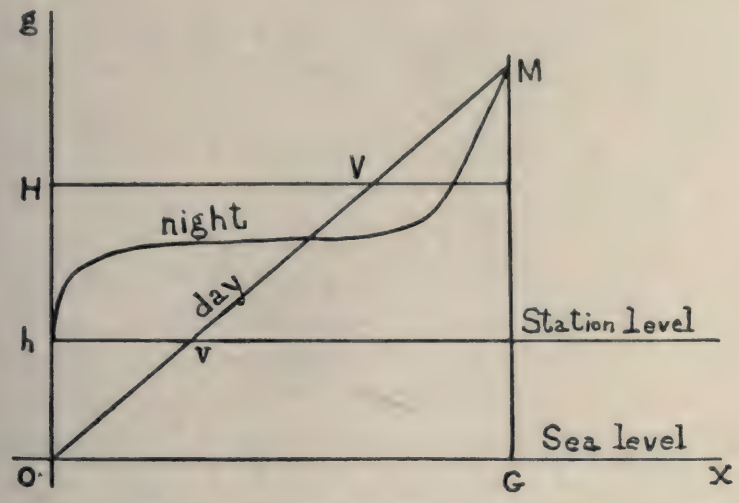


FIG. 5.—Variation of wind velocity with height by day and by night (hypothetical).

of comparative calm we may find a current from an entirely different direction, perhaps a complete reversal or a current at right angles to that of the surface layers.

Without going into detail, let me say that south-westerly, westerly and northerly winds are capable of giving examples of the type of solid current, gradually veering strong winds and continuous increase of wind with height. North-easterly, easterly and south-easterly winds often have their place taken aloft by winds from other quarters, generally with a calm layer interposed between the two currents. A northerly current above a southerly one sometimes occurs.

There is a fifth class in which the wind in the lowest layer is apparently not related to the gradient wind. Such cases are of importance from a meteorological and dynamical point of

view. How far the anomalies are due to the fact that the determination of the gradient may be at fault, on account of minute local differences of pressure, is a question to which at present a full answer cannot be given. Land and sea breezes at the surface near the coast, which have apparently no relation to the barometric gradient, may perhaps be connected with local differences of pressure not shown on an ordinary chart (1).

The gradient wind is doubtless different at different levels because the quotient gradient divided by density may change as we go up. Nor can we assume that the actual wind is necessarily represented by the gradient wind, even when the local variations of pressure are allowed for. There are meteorological conditions in which the scheme of pressure distribution is changing and the steady state is disturbed by causes of which we cannot specify the details. Hence, above our heads, we may have a criss-cross of currents some of which our local observations will account for; the causes of others are beyond our cognisance.

The study of the motion of clouds often gives evidence of currents at different levels in different directions and the observations with pilot balloons show us that there are crossing currents at different levels without any clouds to indicate them. The pilot balloon would be invisible if there were clouds; the observations of pilot balloons and clouds are thus complementary.

Let us go on to consider some local peculiarities in the structure of the atmosphere. We have seen that there are layers of different velocity at different levels. We cannot suppose that the layers are always kept distinct. The air of one layer may pass upward through a layer above, in consequence of some unstable distribution of density due perhaps to warming or cooling either by conduction or dynamically. The cumulus cloud is itself evidence of rising air; the visible condensation of the vapour is in fact attributed to that cause.

The study of the floating of clouds would be a very instructive exercise for a balloonist, because the causes which produce or vary the ascent of what we may picture as a large globe of air are included in those which affect the balloon. The balloon has others in addition, so that the case of a floating cloud or mass of air is simpler than that of a balloon. But the case of a cloud requires some close consideration.

To give an illustration, imagine a layer of detached clouds floating in the middle of a layer of clear air 2 kilometres

thick (fig. 6). The temperatures on the absolute scale may be represented by the numbers 280°, 275°, 270° written against the different levels. The clouds may seem to float very peacefully in the empyrean like so many balloons. But suppose that, either by some mechanical process or the thickening of the surface layer, the whole layer is slowly lifted through a hundred metres and in that way or in some other the pressure above the cloud layer is reduced by the corresponding amount: consider what happens. By the diminution of pressure over the whole surface by about a third of an inch of mercury, the temperature of the cloudless air falls 1°, that of the cloud globule by only ½° because of the condensation. The cloud becomes warmer than its surroundings by ½°; it will therefore begin to rise; but its place cannot be taken by the air just above and around it, because the descending air would be warmed 1° and be too hot

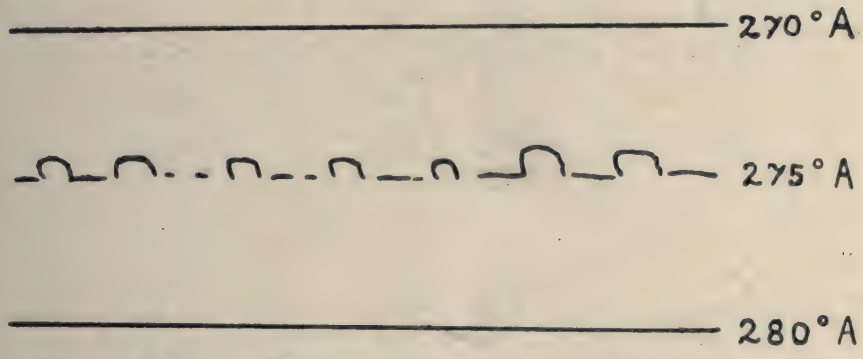


FIG. 6.—Layer of detached clouds between two layers of clear air.

for its environment, so the descending air will have to be distributed over a large area in order that no portion may be warmed more than ½°. The ascent is localised, the descent must be distributed. Hence the cloud patches persist and increase. Reverse the process and compress the layers: on account of the evaporation the cloud is warmed less than its environment, the cloud mass will sink locally, the displacement must be distributed.

So clouds must be regarded as evidence of regions of ascending or descending air, according as they are forming or evaporating: thus the complexity of vertical structure may be considerable.

Let us take another case of local significance, the cliff eddy, which can easily be demonstrated in the laboratory by a wooden cliff-shaped structure and an air-blast. Suppose we have a

current of air blowing on a steep cliff face (fig. 7). We get a vigorous ascending current and, at the top of the cliff, an eddy of remarkable properties. Let any one try the experiment by standing on a cliff edge which faces a gale of wind; he will find that he can throw his hat over the cliff without losing it. An empty match-box is cheaper—that often comes back to him after describing a vertical circular course. In the laboratory small balls of cotton wool answer admirably. Under the influence of the eddy they are first drawn towards the cliff and then thrown upward by the current.

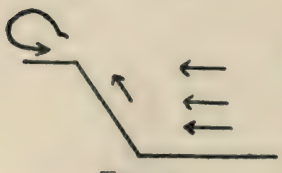


FIG. 7.

Next let us take some important phenomena which depend upon the want of homogeneity of broad air currents. The

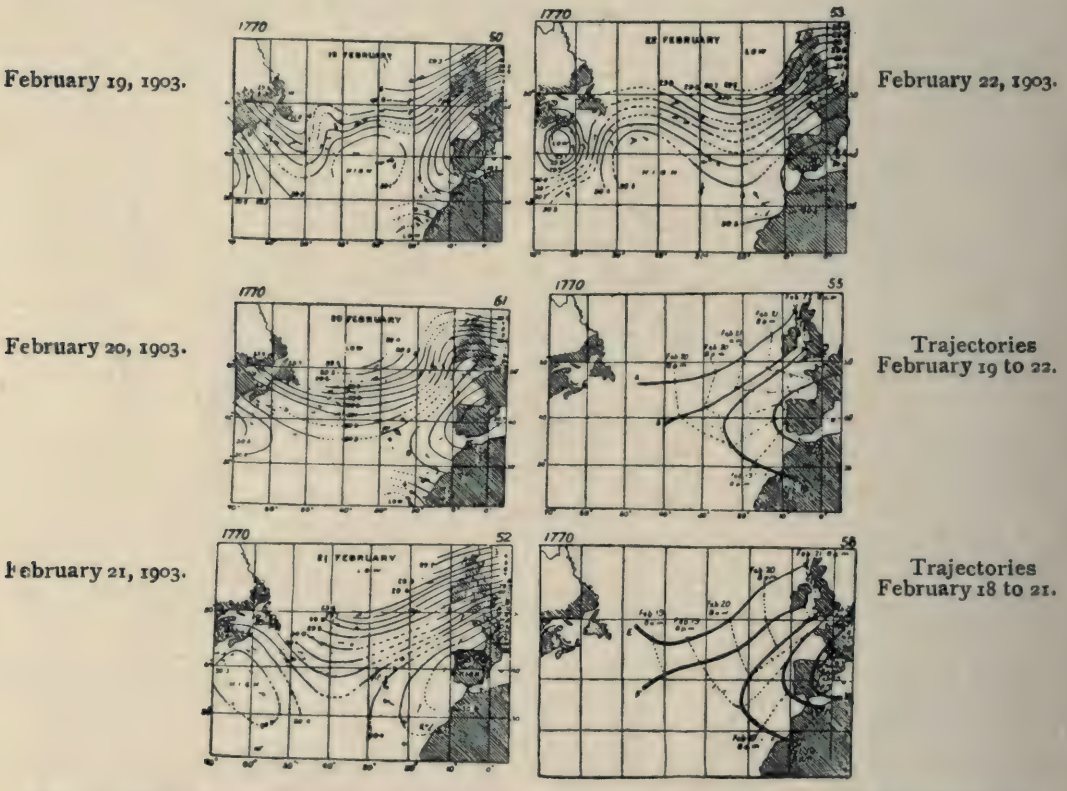


FIG. 8.—Parallel streams of warm and cold air over the British Isles in February, 1903.

north and south sides of a broad air current are sometimes fed from different sources and thus supplied with different air. The thermometer is the best instrument for detecting the want of homogeneity; we may reasonably assume that a sudden but persistent fall of temperature means drawing the air from a colder supply.

We have a very good example in the case of the convection of dust from Africa, in 1903 (6), which is represented in fig. 8. The charts for February 19, 20, 21 and 22 show the distribution of pressure over Europe, North Africa and the Atlantic. The other two charts show the lines along which air may be inferred to have flowed during the four days from the north of Africa on the one hand and from Canada on the other; the two streams were brought into parallelism over the British Isles and the invasion of the territory of the warm current by

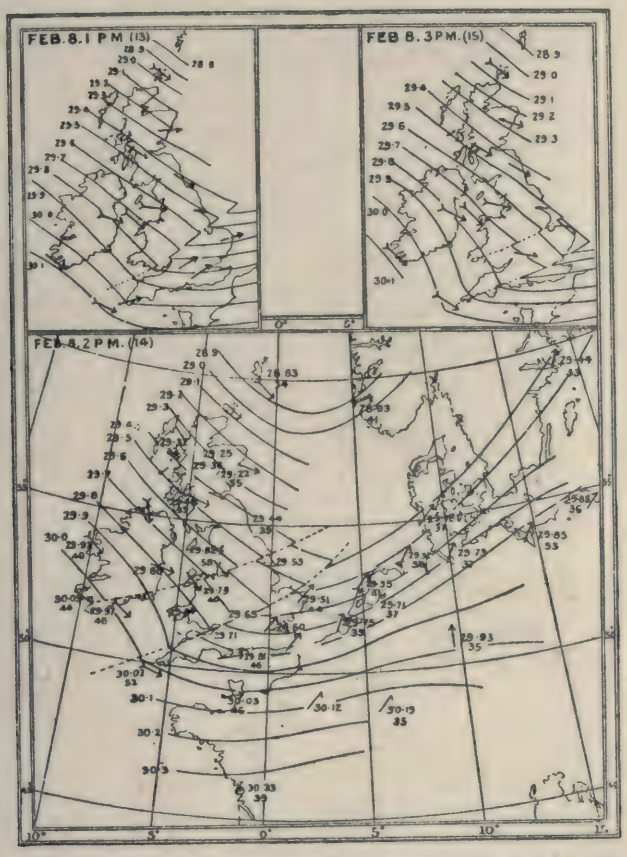


FIG. 9.—Charts of isobars for 1 p.m., 2 p.m., and 3 p.m. on February 8, 1906, showing the positions of the primary and secondary line-squalls at those hours.

the air of the cold current explains the occurrence of the rainfall which brought down with it the African dust.

It appears to be the condition of things thus arranged that gives rise to the phenomena of line-squalls, which we may illustrate by the example of February 8, 1906 (7), represented by the diagrams (figs. 9, 10, 12). Similar diagrams have been prepared for October 14–15, 1909 (8). Fig. 9 shows the distribution of pressure at three consecutive hours. The dislocations of the isobars show the positions of the sudden

variations of pressure which are characteristic of a line-squall and are shown on the maps arranged in lines athwart the isobars marking the position of the squall. Fig. 10 shows the phenomena recorded at Kew Observatory during the passage of the squall. It enables us to note the characteristic events which identify a line-squall: slight rise of pressure, marked fall of temperature, sudden veer of wind (generally a violent gust at the onset) and a sudden shower of rain. Fig. 12

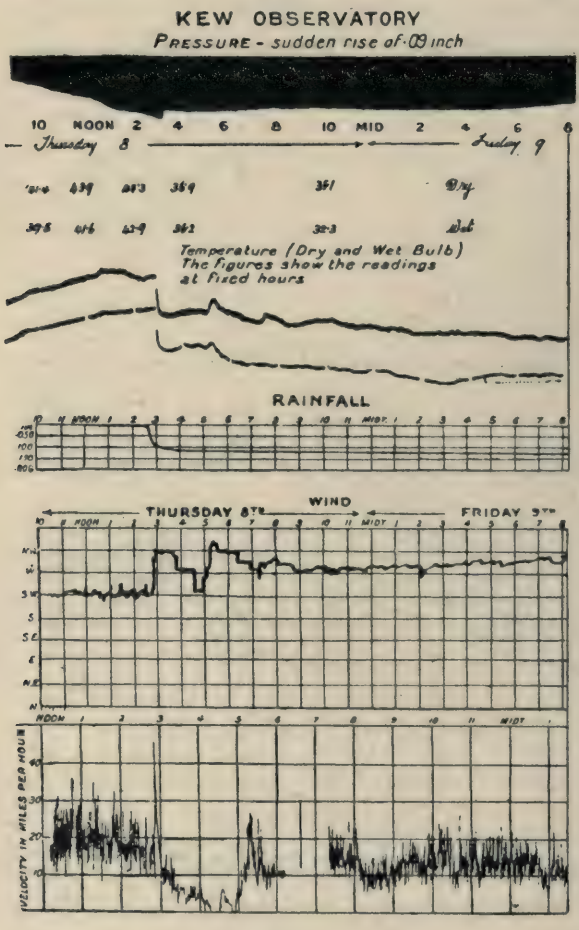


FIG. 10.

shows the isochronous lines for successive hours which mark the advance of the squall with a linear front across the map from north-west to south-east.

These examples of line-squalls are very conspicuous cases of phenomena of quite frequent occurrence in south-westerly, westerly and north-westerly weather. The gentlest example that I know is the narrow band of high cloud at Aberdeen, represented in fig. 11, which was accompanied by an appropriate change of wind; the severest example was in the storm

FIG. 11.—Band of cloud passing from the horizon over the zenith at Aberdeen. (Sketch by G. A. Clarke.)

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which destroyed the Tay Bridge on December 28, 1879. Line-squalls or phenomena closely analogous thereto apparently occur in all parts of the world.

I have already said that in Greek mythology the line-squall

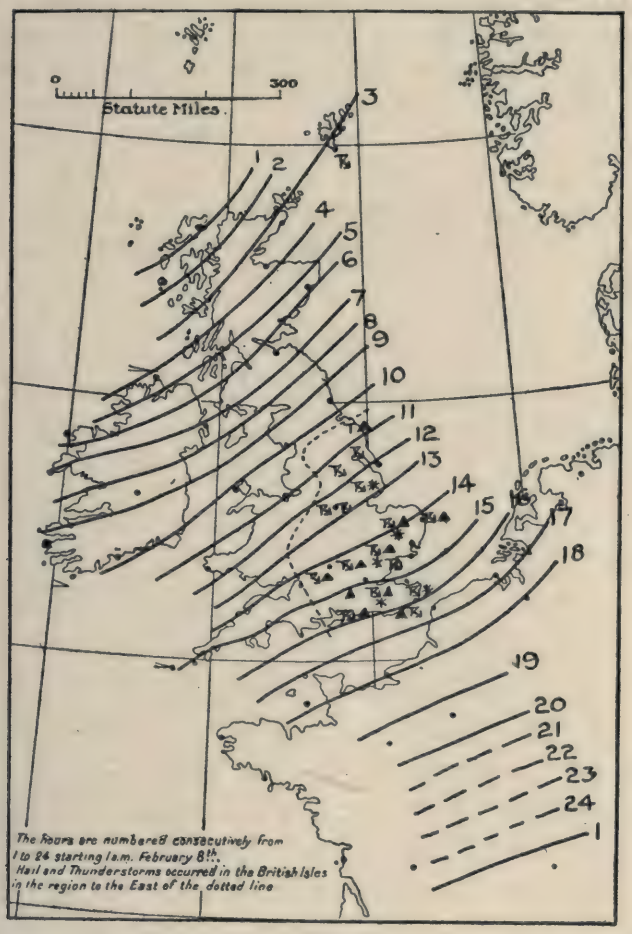


FIG. 12.—Isochronous lines showing the travel of the front of the line-squall from the Hebrides to Central France. The region of thunderstorms in England is marked off by a dotted line.

had a place, being personified by the Harpies. Let me quote what Ruskin says about the matter in *The Queen of the Air*:

". . . I must go on at once to the fable with which you are all generally familiar—that of the Harpies. This is always connected with that of Boreas or the north wind, because the two sons of Boreas are enemies of the Harpies and drive them away into frantic flight. The myth in its first literal form means only the battle between the fair north wind and the foul south one: the two Harpies, 'Storm-swift' and 'Swift-foot,' are the sisters of the rainbow—that is to say, they are the broken drifts of the showery south wind and the clear north wind drives them back; but they quickly take a

deeper and more malignant significance. You know the short, violent, spiral gusts that lift the dust before the coming rain: the Harpies get identified first with these and then with more violent whirlwinds and so they are called 'Harpies,' 'the Snatchers' and are thought of as entirely destructive; their manner of destroying being twofold—by snatching away and by defiling and polluting. This is a month in which you may really see a small Harpy at her work almost whenever you choose. The first time that there is threatening of rain after two or three days of fine weather, leave your window well open to the street and some books or papers on the table; and if you do not, in a little while, know what the Harpies mean and how they snatch and how they defile, I'll give up my Greek myths."

The small Harpy to which Ruskin refers is probably the shower-squall or comparatively mild type of line-squall, characterised by its slight rise of pressure, change of wind (with a squall), fall of temperature and shower of rain. Whether or not the idea of pollution may be associated with the notable darkening of the sky which is very characteristic of these showers I must leave to some one sufficiently familiar with Greek myths.

Thus we may summarise the structure of the atmosphere: At the surface a current running, up to 80 miles an hour in very exceptional cases (with eddies formed by all suitably shaped obstacles in the way of the flow) generally increasing with elevation in proportion to the height above sea level up to the gradient wind; and above that level, different kinds of structure on different occasions—from the "solid" current, which keeps the same speed and direction at all levels, to one which, at higher levels, within 10,000 ft. shows winds of increased velocity or a calm or a reversal or a cross wind; the vertical structure disturbed by local ascending or descending columns of air the positions of which may be indicated by detached clouds, between which there must be a distributed adjustment of level in operation in order to make up for the vertical displacement of the local column: the broader currents made up of heterogeneous streams running alongside, which sometimes, from their juxtaposition, give rise to the destructive phenomena of the line-squall.

Texture of Air Currents

For the texture of air currents we may appeal to the records of pressure tube anemometers which show by the breadth of

the "ribbon" the coarseness of texture of the currents. We may quote from the Wind Structure Report of the Advisory Committee for Aeronautics (3) the breadth of the ribbon which represents the texture at different observatories for a thirty mile average wind.

Anemometer.	Width of the "ribbon" on a pressure tube record for a wind averaging 30 miles per hour.	Range of gusts expressed as a percentage of the average.	
Marshside, Southport .	10 miles	30 per cent.	
Scilly	15 „	50 „	
Shoeburyness . . .	10 „	30 „	for an E.N.E wind
„ . . .	25 „	75 „	for a W. wind
Holyhead . . .	15 „	50 „	
Pendennis . . .	8 „	25 „	for a S. wind
„ . . .	16 „	50 „	for a W. wind
Aberdeen . . .	30 „	100 „	
Alnwick	25 „	75 „	
Kew	30 „	100 „	

It is evident from this table and from the records upon which it is based that the gustiness of wind depends upon the locality; an open exposure on the sea-coast shows for sea winds the least gustiness; from an inland exposure the wind is gusty from all quarters. Some examples of "ribbons" are given in fig. 14.

The question of the variation of the gusty texture of the wind at higher levels is one to which no complete answer can yet be given. For the Meteorological Office, in connection with its work for the Advisory Committee, Mr. J. S. Dines (11) has made out the gustiness aloft by recording the length of kite wire and the variation in the pull of the wire at different heights, making allowance for the effects of the long lengths of wire in cushioning the variations of pull. From his results for thirty-four kite ascents at Pyrton Hill we may make the following rough generalisation:

PERCENTAGE GUSTINESS AT DIFFERENT LEVELS, TAKING THE GUSTINESS AT 0–500 FT. AS 100

Height: 0–500 ft.	500–1,000 ft.	1,000–1,500 ft.	1,500–2,000 ft.	2,000–2,500 ft.	2,500–3,000 ft.	3,000–3,500 ft.
100	58	40	46	33	30	27
No. of ascents: 34	22	11	17	6	13	6

The results for each level are very variable. On one occasion with a westerly wind the gustiness at 1,500-2,000 ft., for example, is given at 111 per cent. of that of the lower strata. But in Mr. Dines's method of dealing with the records, the phenomena of recurring squalls are included in the ordinary gustiness.

Mr. Dines has also compared the records of wind at a height of 98 ft. and at 35 ft., 150 yds. distant. He finds that groups of gusts may be shown upon the one anemogram which have not affected the other. Hence in considering the texture of wind, we must distinguish between the general gustiness which is

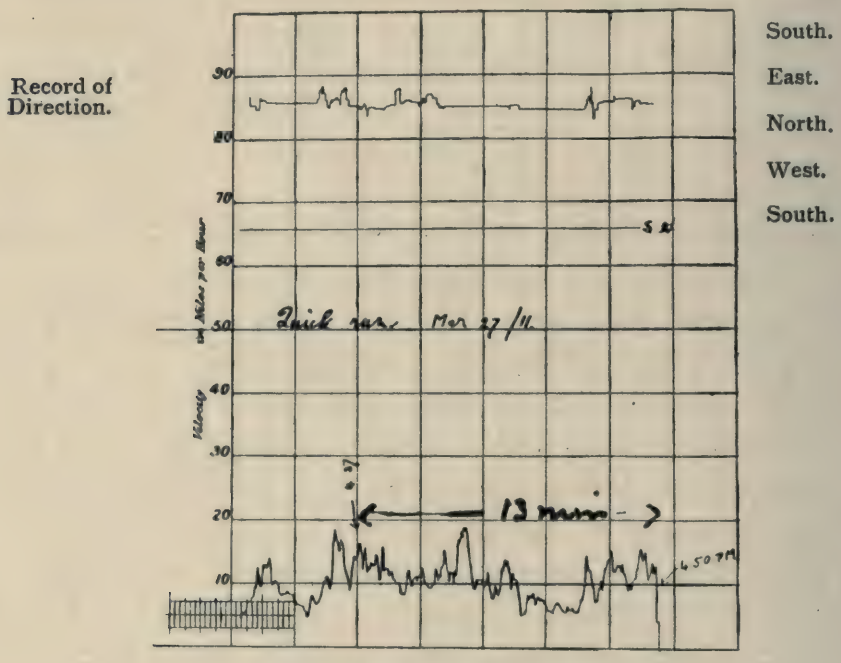
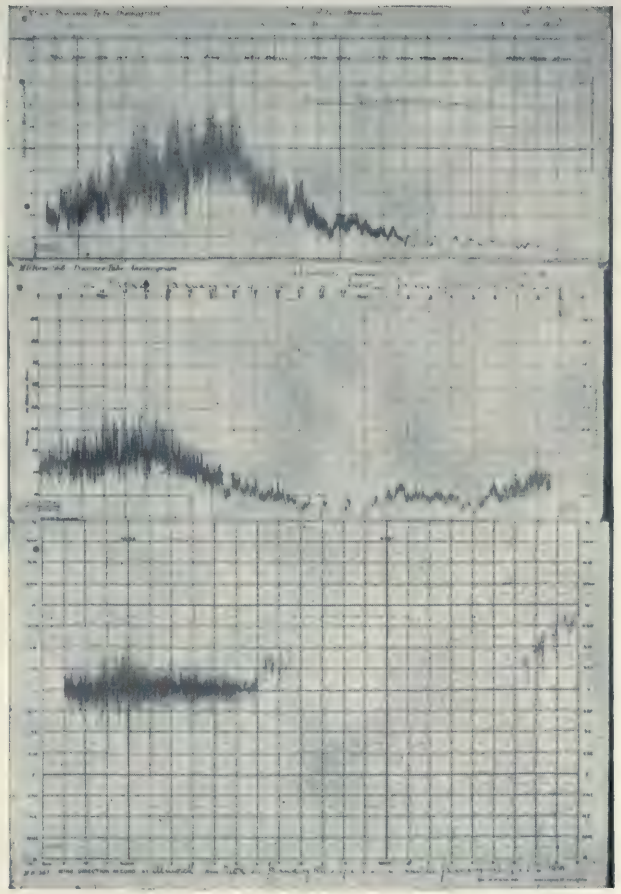


FIG. 13.—Record of quick run.

more or less regular in its irregularity and the special gustiness of local groups or squalls.

An analysis of the ordinary gustiness may be shown by the record of a quick run which Mr. Dines has obtained. It is represented in fig. 13. The fluctuations in the wind shown on the diagram are those which, with a certain reservation, we may call waves. When we compare them with the waves of the sea there is really little analogy. The gustiness is not regularly periodic like a succession of waves and the magnitudes of its excursions are equally irregular.



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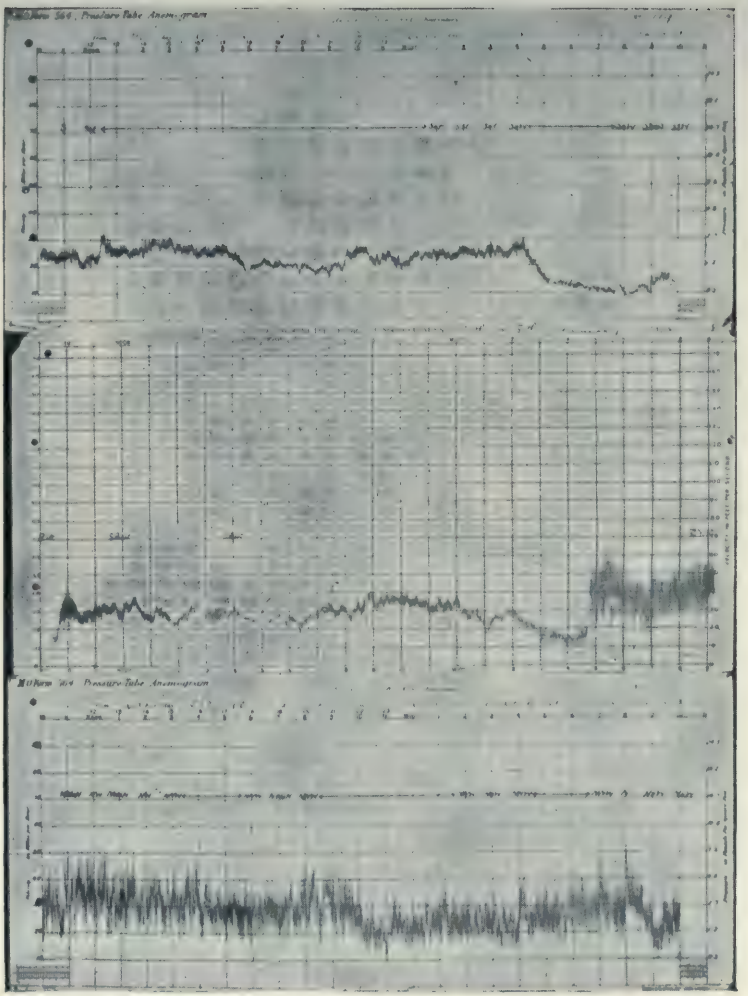

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FIG. 14.—Examples of different kinds of "ribbon" shown on the records of pressure tube anemometers.

1. Aberdeen.
2. Alnwick (velocity).
3. Alnwick (direction).
4. Pendennis Castle (sea wind).
5. Shoeburyness.
6. Pendennis Castle (land wind).

Conditions of Safety of Floaters and Fliers

In taking up the question of the conditions of safety I shall deal only with that part of the subject which is concerned with the meteorologist's discretion and not with the airman's valour. I could perhaps have made myself more clear by selecting as title "The Dangers of Floaters and Fliers"; but even that title would not have been exact, because there is really no danger in a line-squall or a cliff eddy if you have the skill to recognise its approach and the discretion to keep out of its way.

One distinction between floaters and fliers is an obvious one. No harm can come to a drifting open boat except from waves, whirlpools or the bottom. A floating balloon seems at first sight to be immune from any sort of injury, so long as it does not come to land. There are, as we have seen, the irregular waves of gustiness to which the balloon cannot instantaneously respond and consequently it must be subject to some stresses; but experience shows that they are not of great importance. It is a little surprising that no one has yet made a machine to take power out of the gustiness of the wind, using the variations of velocity to drive a pump instead of the average velocity. It is obvious that a mechanism could be made to pump water by the gustiness of the wind and it would be very interesting to know what sort of work an apparatus of the kind would show in a balloon. I have seen no notice of any effect of gustiness on the motion of a balloon and we may say that the "waves" of our anemometric records are not a danger to a floater.

It might appear indeed as though a floater would travel serenely along and carry its own weather with it. If it starts with a cloud it might travel with it as though the cloud were a sort of umbrella; if it is raining or snowing the balloon might be expected to travel with the rain or the snow; if it does not there must be some complication of the structure of the atmosphere giving a different régime of weather in the upper air from that of the current in which the balloon floats. The consideration of the immunity of a floater from the incidents of weather, as experienced at a point of the fixed earth, is of some practical importance in modern aerial navigation, because an airship can at any time become a floater and drift with the air current that supports it. I have therefore been curious to find out whether a line-squall, which may set up a sort of destructive

whirlpool in the atmosphere, could catch a balloon. I have watched this subject carefully since Mr. C. F. Pollock and Mrs. Assheton Harbord made a journey in a strong north-westerly wind from London to the Continent in a free balloon. They were brought to earth in a forest by a squall accompanied by snow which tilted the balloon and loaded it with snow, so that they had to descend in circumstances of great difficulty, fortunately without mishap. It seemed difficult to account for a squall catching a balloon and in consequence I have taken advantage of opportunities to arrive at some reasonable explanation of the unpleasant fact that a line-squall can catch a floater.

We have examined the details of a number of line-squalls in the Meteorological Office (8) and I am now prepared to offer a suggestion as to the course of procedure by which the result is arrived at (9). Let us go back to the maps of a line-squall. We find that the line of the squall is athwart the "following" wind, *i.e.* the wind behind the squall front; if we take a section across the line of the squall we have the following wind with its velocity v and the wind in front which is inclined at a finite angle to the following wind; if we take only the *component of the wind at right angles to the squall front*, that also appears to be of the same magnitude, viz. v.

But the line of the squall itself advances with a velocity somewhat greater than the surface wind behind it; it therefore leaves the surface wind behind and sweeps away the wind in front. The question is, How is this managed? Let us take into account the undeniable fact that in the upper air, over both front and rear, the wind velocity is greater than that of the surface wind and is, in fact, to be represented at a moderate height by the gradient velocity, which we may take to be about twice the surface velocity. Let us suppose that the velocity of advance of the squall is half-way between those of the surface wind and the gradient wind. Hence we get the state of things representing the motion in the vertical cross-section across the squall line. We have surface wind with velocity v, upper wind with velocity $2v$, and the line-squall, marching with a velocity $3/2\ v$, gaining $\frac{1}{2}\ v$ on the front and leaving the rear wind behind by the same amount. The air behind is colder than that in front.

Now clearly the upper air is gaining on the surface air and the crest of the advancing cold wave must topple over, driving up the warm air and producing rain or snow and sweeping away

the warm air in front of it. Thus we have the advancing breaker of a bore and the way in which it can catch a balloon is represented by the diagram (fig. 15). The first effect upon the balloon is its elevation with the air which is being pushed up; the second is the loading of the balloon with rain or snow; the third stage is the down-rush of the toppling crest which falls earthward and produces the sudden squall that is characteristic of some of these phenomena. But we must not forget that our diagram only represents a part of the velocity of the air in which the balloon was originally floating and that, besides what is shown in the diagram, there is the velocity parallel to the front which is suddenly arrested as the squall breaks and the balloon finds itself in a region of very turbulent motion which has actually caught it as it floats.

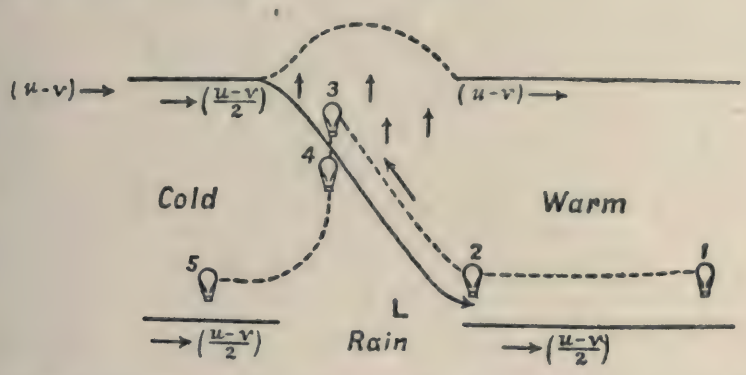

FIG. 15.—Illustrating the motion of air in the vertical section across the front of a line-squall.

If this be a true description of the phenomena and all the observations seem to confirm it, we must conclude that on the approach of a line-squall there is no safety for an airship in floating. It must run away if it can. It can run away successfully if it can travel at right angles to the line of the approaching squall with a velocity half as great again as the normal component of the surface wind. It must, however, be remembered that the actual proportions of the velocities here mentioned are hypothetical until we are fortunate enough, in some way or other not yet clear, to obtain measurements which will enable us to verify the explanation here suggested.

When the balloonist is tired of the amusement of dropping imaginary shells upon imaginary ships, we can find occupation for his leisure in keeping an accurate log of the changes of weather above a floater.

For fliers we have to deal mainly with the texture of the air current. It is clear enough that the only condition of safety with respect to a cliff eddy or the eddies due to other obstacles of various kinds is that they must be avoided and localities for alighting selected which are sufficiently far removed from such occurrences. We turn our attention therefore to the gustiness of ordinary winds.

The gustiness must be for the flying-machine what a bumpy road is for the cyclist or a bumpy sea for a small boat. In consequence of the variation of velocity above mentioned, the airman in a gusty current must be prepared for constant attention to the maintenance of his level against variations of lifting force and to his longitudinal stability; in so far as the gustiness is dependent upon exposure and upon height above ground the conditions will be varying constantly with variations in the ground contours and the shape of the ground.

Airmen have become accustomed to speak of so-called pockets or holes in the air as offering unexpected obstacles to aviation. We have at present no meteorological name for a hole in the air and do not know the peculiarities of the air currents in it; we do know that gustiness, represented by change of velocity above, must offer difficulties in the way of alteration of vertical force and of longitudinal stability and that gustiness, as measured by the range of velocity in a few seconds, though very irregular, is roughly proportional to the average velocity and depends upon exposure. When, therefore, in a flight, a position of stronger and gustier wind is reached it is inevitable that a greater strain should be placed upon the airmanship of the pilot and the aeroplane may easily get beyond his control.

We do not as yet know the limits of gustiness which different forms of aeroplanes can stand without becoming unmanageable; we may, however, feel sure that there are limits of gustiness which it is unsafe for an airman to attempt to navigate and we want the regular comparison of the experience of the airman and the records of gustiness obtained from anemometer records near the ground and by kite records higher up.

The first stage of this inquiry is to find out more exactly what the gustiness really amounts to. I have spoken of it as represented by variations of velocity only but we want to know how far that is a true representation. An anemometer which

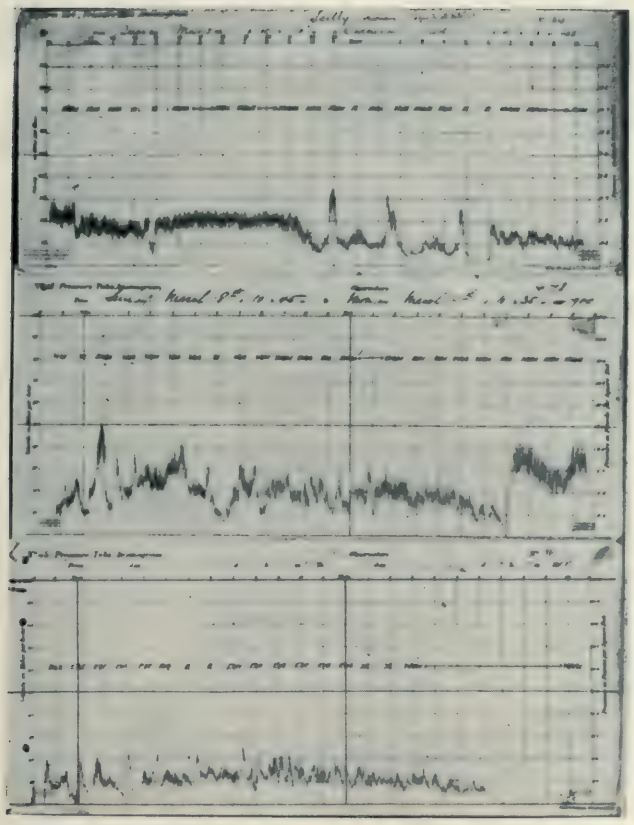

FIG. 16.—Recurrent squalls.

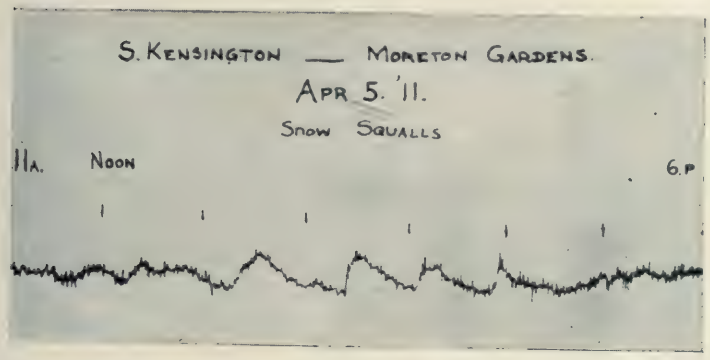

FIG. 17.—Record of pressure variations during recurrent snow squalls.

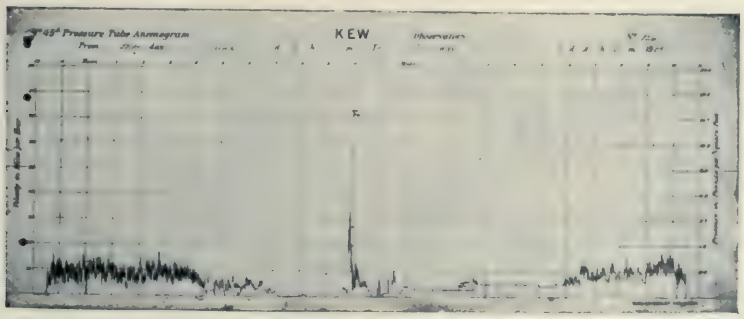

FIG. 18.—Record of wind for the thunder-squall at Kew, June 1, 1908.

records variation in direction shows a trace very similar in character to the trace of the pressure tube anemograph which records velocity. See the record of direction for Alnwick, fig. 14.

Suppose that the surface current is something of the nature of a vast stream of water flowing along a channel, what are the changes in the direction and velocity?

That there are frequent and irregular changes in velocity and equally frequent and irregular changes in direction we know. We want to find out how these are related (10), because if a change of velocity is accompanied by a change of direction the lateral stability of the machine has to be considered as well as the longitudinal. In order to examine this point the Advisory Committee for Aeronautics has found means for me to have an investigation carried out by Mr. J. S. Dines

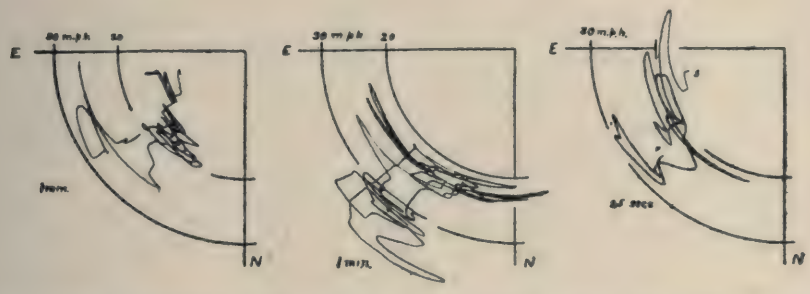

FIG. 19.—Vector diagrams of wind at 98 ft. for 1 min., 1 min., and 25 sec. respectively, showing irregular fluctuations in direction and velocity.

at Mr. W. H. Dines's establishment for the study of the upper air, in connection with the Meteorological Office, at Pyrton Hill (11). Mr. J. S. Dines has worked out the design of an instrument by which direction and velocity of the wind at any moment is recorded as a single point on a diagram and the complete diagram, which we call a vector wind diagram, represents the sequence in the direction and velocity of the wind during the period that the instrument is allowed to operate. Naturally there are some instrumental difficulties. It is not certain how far the record of a pressure tube anemograph in a wind that is varying very rapidly is a strict record of the velocity of the wind; all the different parts of the apparatus have momentum and momentum interferes with the fidelity of the record. The same is more obviously the case with the wind direction. A sudden gust may set the vane in motion but how far it turns depends in part on its momentum.

These difficulties are to be got over by damping, which delays the effect of the wind but excludes the effect of momentum. We are not yet at the end of our experiments on this point but I give here some of the results obtained. I may say at once that irregularities are as pronounced as the most pessimistic investigator could anticipate. Fig. 19 may be appealed to with confidence in justification of this statement.

We have divided all the diagrams obtained from the vector apparatus into four types:

Type I. When the wind is steady and no change of importance takes place either in velocity or direction.

Type II. When the changes in velocity and in direction

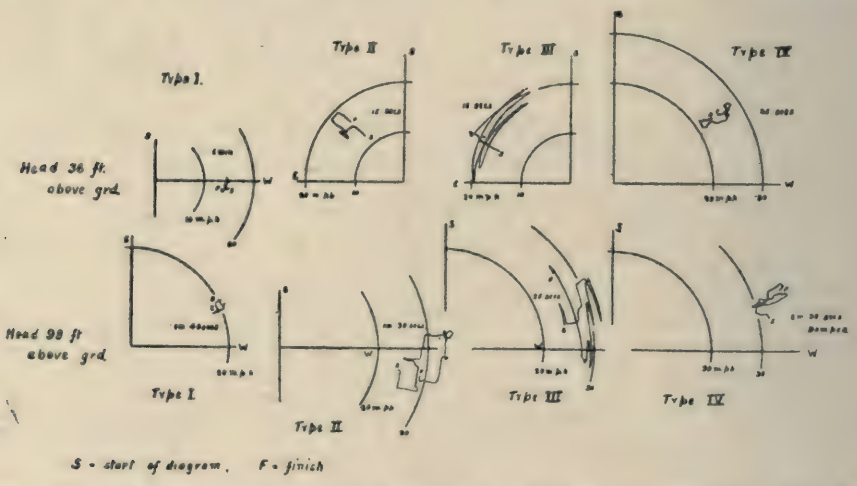

FIG. 20.—Four types of vector diagrams of wind changes during intervals from 15 sec. to 2 min. 30 sec.

are independent of each other. This is a common type when the apparatus is undamped and gives rise to diagrams of rectangular shape.

Type III. When the changes of direction are large and rapid but accompanied by small changes in velocity giving rise to diagrams with sharp V's.

Type IV. When both velocity and direction are subject to damping and a fairly smooth oval diagram is formed.

These types are represented in fig. 20.

We may pursue this investigation in co-operation with airmen who can estimate the difficulties of manœuvring in different conditions recorded by an anemometer or other special apparatus, until we can assign a limit of gustiness in which it is not safe for an air pilot to take his craft. We have seen that the

limit depends upon the locality and on the velocity of the wind and, to go more into detail, on the direction of the wind and the height above the ground. The diagrams suggest 15 to 20 miles per hour as the limit of safety in ordinary gustiness.

Besides the disturbances due to ordinary gustiness the airman may have to deal with line-squalls and recurrent squalls such as those represented in the anemograms for Scilly, March 4, 1908; Holyhead, March 18–19, 1900; Aberdeen, December 31, 1907, to January 1, 1908, shown in fig. 16 and in the barogram for South Kensington for April 5, 1911 (fig. 17). We must not forget the local isolated squalls characteristic of thunderstorms of which the one represented in fig. 18 for June 1, 1908, is a very noteworthy example.

It is to be feared that a line-squall or thunder-squall is essentially unsafe for any air craft except a floating spherical balloon with a very large reserve of lifting power and even for that it furnishes an experience which is better avoided. (See an account of such an experience in Blasius on "Storms.") We do not know enough of recurrent squalls to make any useful statement about them except this, that they occur especially in the western section of a cyclonic depression; the occasions when the changes incidental to that part of a depression are in progress are not suitable for trial trips, nor even for the ascents of experienced airmen.

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THE INTERPRETATION OF LIFE[1]

By F. CARREL

Although research has thrown much light of late upon the phenomenon of life, our knowledge is still sufficiently limited to permit of the existence of two main schools of thought each offering a different interpretation of its meaning. The smaller and less active school may be said to be a descendant of all the animistic thought which has preceded it: the creed it adopts is now known as Vitalism, a doctrine according to which the life process is under the guidance or direction of a certain animating influence or principle. The larger and more active school is that of biological determinism, which pursues the experimental method and tends to teach that life is due to chemical processes, a section holding that these may eventually be reproduced.

Modern vitalism has various exponents, some more and some less metaphysical in the character of their minds. The former aim at proving that life does not exist by virtue of the forces which apparently combine to produce it but because of the direction which is given to them from an extraneous source; the latter consider that the life phenomenon is chiefly distinguishable from the physico-chemical by the form which it assumes.

One of the best exponents of the theory of direction, who also uses at times the argument of form, is Grasset. According to this writer, there are in the human body certain evidences of an extraneous influence which guides, regulates and maintains existence. We know through physiology that although we are unconscious of the working of our internal organisation, activities are in operation within our bodies which maintain life. This condition of things Grasset regards as due to some influence other than that of chemical change; he adduces in support of this view the facts that the blood and the interstitial liquid of the tissues and organs are in a fixed osmotic equilibrium, always preserving the same force of diffusion and exchange with the exterior environment, the agent of the

[1] We shall probably comment in a later number on the issues raised in this article.—Eds.

equilibrium, chloride of sodium, remaining constant whatever the quantity of salt introduced with food. He further points out that the distribution of sugar is maintained in accordance with the needs of the organism, whatever the quantity consumed; that reproduction is an essential characteristic of life which has never been imitated by chemical synthesis; and that local life or the separate activities of the various constituent parts of the body is an evidence of design—all the local lives as well as the reception and restitution by the nervous system of exterior energies being co-ordinated and regulated by and for the life of the individual. There is, he declares, functional unity with an extreme organic complexity, the significance of these facts being further strengthened by the propensity of organisms to struggle against environmental influences inimical to them, against nutritional excesses and above all against the microbes by which they are assailed and as a protection against which they are provided with vibratory cilia, gland secretions and other means of self-defence. There is said to be room for a physical vitalism which takes into account what is of a special nature in the manifestations of life and what is in conformity with general forces. What is special is stated to be the directing idea, which is that of the preservation of the individual and the species.

Now the determinists deny or rather do not admit the theory of guidance. Claude Bernard, one of the founders of experimental biology, laid it down that the manifestations of life are due to two factors: (1) pre-established laws which regulate phenomena in their succession, concert and harmony; (2) determined physico-chemical conditions which are necessary to the appearance of the phenomena. Dastre, the pupil of Bernard, defines life as the function of extrinsic variables: water, heat, the chemical composition of the environment, pressure; whilst Spencer contended that life was the continuous adjustment of internal to external relations.

It is evident that if the series of phenomena which constitute life were not governed by the laws by which chemical affinities and interactions are determined as well as by those which regulate the properties of bodies, osmosis and diffusion, the fluidity or viscosity of fluids, electricity both internal and external, thermal and hygrometric conditions, all co-operating to maintain the life of the organism: then, things being as they are, life

could not exist. In other words, if there were not a co-operation between the forces which constitute phenomena in the outer world and those which co-ordinate them in the organism to support what is called life in living beings, there would be no living beings. Life is a condition of this co-operation. The co-operation itself, however, in its completest form, *i.e.* in the higher animals, is the sequel of the development from the ovum in which it can only be said to be potential, since at the outset of the animals' existence only a small number of the life processes are operative which are subsequently brought into effect. So that general arguments tending to prove design should take their starting point at the origin of life, because the subsequent complexities are the results of the potentialities contained in the protozoic substances.

But if there was only one such substance at the origin, it would be necessary to credit it with an infinity of latent and divergent energies. A multiplicity of definite designs must, on this hypothesis, have been potentially included in the original material. But such a concentration of potentiality would probably long remain without the scope of experimental science.

Although as the physiologist already alluded to declares, there are certain facts revealed especially in the human body—fixed osmotic pressure, the constancy of the quantity of sodium chloride and of sugar, the process of oxidation—which tend to prove that the body with its local lives co-operating in one harmonious whole maintains an equilibrium in its exchanges; there is scarcely more reason to attribute these to a special regulating power than there would be to assign to the same power the rejection of food taken in excess of assimilative needs by the peristaltic action of the intestines and colon or the practical invariability of the temperature of the body whatever the temperature of the outer air. It is possible to find many circumstances which appear at first sight to point to guidance but when it is considered that no organism can live independently of the environment (air, water, heat), it seems plain that the appearance is an illusion and that no free principle of life exists. What may be called the chemical subservience of the body to its vital needs is the *conditio sine qua non* of life but this subservience cannot be completely understood until more light has been thrown upon the precise nature and behaviour of the enzymes which play such an important part in the somatic activities. All

through nature the living thing takes from its environment the substance which it needs in the measure that it needs it, rejecting what is superfluous or noxious. The organism resembles a cistern which is constantly being filled but does not overflow on account of the valves with which it is provided. It is as useless to ask why it has these valves as to inquire why the force of gravity exists to preserve the globe in its position in the solar system. These secrets are hidden in the ultimate nature of things.

No essential principle of life can be extracted from a living body by any method yet discovered and there is not a sign that any such extraction will be made. If life were something separate from the living body, it would presumably be something which would not suffer change of quantity. There would not be less of it in old age than in youth (at least in the higher forms). The contrary, however, appears to be the case. And if it does change, in quantity, what, it may be asked, becomes of the difference between the maximum and the minimum in the body of a single individual? How is the law of the conservation of energy applicable here?

In seeking for what is special, the vitalist is obliged to enter the psychological domain and to fall back upon the "directing idea," which is the idea of the preservation of the individual and the species, in other words the tendency of the whole being in its conscious and semi-conscious elements to realise existence. What is received from the outer world is reproduced, as Grasset admits, in other forms in the organised body. The organic and the inorganic are bound together in an indissoluble connection. It is probable that the vitalist conception is often due to the too exclusive study of life in its higher forms. The elaborately organised human body, for instance, with its complex series of interactions, fermentations, osmotic and circulatory phenomena, may appear to some to work in obedience to a hidden impulse outside of or beyond the natural laws we know but if life be considered in its most rudimentary conditions, in the living specks which the microscope reveals, the suggestion of design—for it is only a suggestion—should appear less strong. And yet life in a microscopic object immersed in its nutritional medium is not different in kind from life in a larger body immersed in air. It is however not quite impossible to connect the microscopic object with the inanimate matter from which it may have sprung, especially if it be remembered that

the latter has now been ascertained to be far less inert in regard at least to the particles which compose it than was once supposed. And if the simple organism, alive in the same sense as the complex is alive, derived its animation from an original germination from the inorganic, it would then seem clear that the complex owed its vital characteristics to the same source. For what is it that brings about the antithesis of life or death? In the lower as in the higher organism, it is the failure of functional concordance and the cessation of a particular series of exchanges with the environment. There is in all organisms a point beyond which lesions or mutilations cannot be incurred without the causation of death and the point is reached when one or more of the organs is cut off without which the functional harmony and the necessary chemical exchanges cannot be maintained. As long as the cerebral, digestive and circulatory processes are at all possible, the organism can live, a considerable time at least, notwithstanding the loss of many of its members. A man can still exist after having been deprived either of both arms and legs, one kidney and one lung, a stomach and a portion of the intestines, because these losses do not put an end to the somatic process. An earthworm may be greatly mutilated and yet continue to exist. Life can be maintained for a time in the lower animals after the excision of the brain, because the bio-chemistry continues even in the absence of the thought organ. If vital principle there be, it is not an independent one but a general concordance of certain materials in a special relation to each other and to the forces of the exterior world. To seek an extraneous origin for life or a guiding principle of life is equivalent to seeking the first causes of the chemistry of nature. Before attempting such a task it is better to be sure that the whole effects of that chemistry have been understood. If life as we know it should be ascertained to depend on certain physico-chemical activities, we should then be justified in thinking that its origin was wholly due to the same forces. And there is little doubt that we are moving to that conclusion. The conditions under which life appeared were no doubt special but the matter involved can have undergone little change and it seems that if we could reproduce the conditions in which life first germinated, we should be able to reproduce at least the form in which it first appeared.

It seems therefore that the endeavour of biology in this connection should be to renew these conditions and to place in them the material of which we see life to be composed, so that the vital synthesis should be complete. To do this may or may not be beyond the power of biology and the sciences it may summon to its aid but it is only by attempting it that biology can hope to pierce the enigma of the state of life.

The work is of enormous difficulty. It has been proved by the experiments of Pasteur that it is useless to provide a purely nutritional medium and to wait for life to appear in it, because whenever the sterilisation has been impeccably performed no life appears. The gelatin or other substance extracted from dead animals does not itself give birth to life even when placed in the thermal conditions favourable to germination. The process of life origination appears to be far more complex and to demand the co-operation of forces that are not present in a test tube filled with sterilised solution of an organic substance.

It has been held with some show of reason, although without any possibility of proof, that the evolution of life was a process occupying an enormous space of time: consequently that it is vain to attempt to make plasm in a few days or weeks which nature employed millions of years to manufacture. Inquirers, however, have not always been deterred by these considerations from attempting to find a short cut to the process and during the last half-century not a few attempts have been made to construct life out of dead matter. These essays are mainly of interest because they tend to focus research into channels where it might be susceptible of leading to results. Let us briefly consider the most recent.

J. B. Burke, in 1905, claimed that by the action of radium in a culture medium he produced objects which were not unlike ordinary bacilli of the same dimensions. The process consisted in mixing radium salts with gelatin, then sterilising at 130° to 140° C. during half an hour. Objects gradually appeared in this medium varying in size from 3 microns to mere specks when seen under a 1/12 inch power. On the borderland apparently between crystals and bacteria, it is said that they developed nuclei after six or seven days, that they ceased to grow and then segregated and multiplied, becoming visible to the unaided

eye, in a mass at least, on the surface of the medium in which they were produced.

Now like all similar growths hitherto witnessed, these productions are the subjects of much doubt. In spite of the care bestowed on their formation, it is hardly possible to be certain of the perfect sterilisation of the medium in which they appeared. It may be that we have still much to learn concerning the resistance of micro-organisms to heat. Although the experiments of Pasteur are generally held to be conclusive, still there is as yet no consensus of opinion as to the exact temperature at which life ceases. It has been suggested by Le Dantec that the radiobes of Burke may be half dead organisms which the radium has had the effect of reanimating and if this were the case, experiments such as these would only prove that life can spring from the slenderest of vital threads. Burke himself did not claim that the objects which he produced were to be positively called living; only that they had every appearance of microscopic beings.

Bütschli in conducting his experiments on microscopic foams and protoplasm did not endeavour to produce life but he was able by means of a *mayonnaise* of olive oil, common salt, cane sugar and water to produce a froth which under the microscope revealed alveolar and other characteristics strongly resembling those of protoplasm and behaving in its streamings, aqueous diffusions, general movements and electrical conditions in the same manner as protoplasm. Here was an attempt to gain an acquaintance with the physical nature of the primal substance by means of an imitation made with suitable materials, an attempt which at least succeeded in showing that the constitution of protoplasm, with its nucleated cells, its flaccidity and its alveolarity, was not a special attribute of life but *in posse*, otherwise, in Nature. By these experiments, a valuable contribution was made to the work of reconstruction which is slowly taking place. For it is evident that the more knowledge is gained of the extension of the physical properties of life beyond the actual spheres of life, the better the synthesis can be attempted from which it is sought to evolve the living thing.

The experiments of Bastian were designed to show that, contrary to expectation and to what has hitherto been considered probable, life may spring from purely inorganic matter under

certain conditions. It is well known that this inquirer has devoted considerable time and labour to the promulgation of a doctrine whereby life, instead of owing its origin to one protoplasm, evolved in the early stages of the world's history and perpetuated in a multiplicity of forms to the present day, can and does appear *de novo* in the world. In support of this, he has made with great pertinacity and conviction a series of attempts to produce life out of some of the elements most commonly distributed over the earth's crust. Out of the deposit formed by a few drops of sodium silicate and of ferric nitrate in an ounce of water in one case, as well as out of the deposit formed by a few drops of the same silicate together with a few drops of phosphoric acid and a few grains of ammonium phosphate, in water, in another case, he claims to have obtained various microscopic organisms—bacteria, torulæ, moulds, etc.—after sterilisation of both solutions at temperatures of 115° to 145° C. during 5 to 10 minutes and the expiration of periods varying from one to twelve months.

It is certain that if life sprang originally from the inorganic matter of the cooling earth, there is no inherent impossibility in the production of life germs from the chemical substances which mostly compose the earth, provided that the conditions have been obtained which first existed and that the necessary synthesis has been achieved. This is far from admitting, however, that the productions of Bastian are synthetic in the biological meaning of the term. It is by no means positive moreover that heating to 145° C. is sufficient to destroy all possibility of germination. According to some views, solutions from which it is sought to evolve life should first he heated at least to 150° C. and kept at that temperature at least thirty minutes. Dr. Bastian, however, states that the higher temperatures "degrade" the saline mixture and tend to destroy what he calls its "germinality."[1] Air-borne germs, also, may accrue after the tubes are opened.

[1] On inquiring of Dr. Bastian by what train of reasoning he was induced to try to obtain his results with these particular solutions, I was referred by him to a passage in his book *The Origin of Life*, 1911, pp. 72 and 73, wherein mention is made of the writings of Royer, Herrera, Renauder, Alb. and Alex. Mary and (especially) Prof. Emerson Reynolds in support of the statement that many silicon compounds resemble those of carbon with nitrogen and that under suitable conditions a plant or animal may be able to construct from silicon compounds "something akin to silicon protoplasm for use in its structure."

The experiments of Leduc, some of which have too lightly been considered as mere scientific amusements, are in reality of interest. By a careful study of osmosis and diffusion in relation to life, Leduc has gained some knowledge of the morphogenetic methods which nature appears to use in the production of living matter. Claiming that all the laws concerning the physical behaviour of gases apply to solutions, he states that osmotic pressure is constantly at work in living beings and that where it ceases to be operative, life becomes extinct. This inquirer has shown that where a membrane of ferrocyanide is interposed between a substance A in a solution B, there is produced an interchange which results in the swelling of A (enclosed in its membrane) and its expansion into forms resembling both vegetable and animal life, forms in which, as long as they last, a constant process of intussusception is in operation. He has exhibited this osmotic phenomenon in a variety of ways and has also furnished a number of instances of the morphogenetic tendencies of solutions of different degrees of concentration when in contact with each other. Forms of living things can be produced at will, in saline solutions, by the guidance of the operator, a guidance, it must be observed, which, qualitatively at least, is exercised solely in the choice of the substances to be dissolved. These substances are largely compounds the elements of which are found to be concerned in the chemical changes occurring in life. Hence there are carbon (in carbonates), phosphorus (in phosphates) and chlorine (in chlorides), whilst among the metals are iron, manganese, potassium, sodium and calcium in different combinations. The last two occur abundantly in nature and it is conceivable that they might have been drawn upon originally for the elaboration of life. Again the phenomenon of growth may be considered to be osmotic since it depends upon nutrition and nutrition itself depends upon the distribution of the nutrient substances through permeation. As in the objects produced by Leduc, there comes a time when the nutritional process ceases to promote growth, after which an equilibrium is maintained during a certain period and this is followed by decline and death. This is the history of all life-growth : stability, destruction. No combinations are eternal. Matter whether dead or living is in a constant state of change. These truths are of course not new but as research progresses they acquire clearer

meanings. As Leduc says, living beings which begin with simple and develop into complex shapes are transformers of form. Also they only lose their acquired forms when they cease to live. If, as some of the experiments above referred to tend to prove, form be acquired by the effect of liquid upon liquid, by the action of crystalloids upon colloids and by diffusion through the membranous substances of living bodies, then it is evident that much attention should be bestowed upon the laws which regulate these actions and exchanges and that this author's claim that the physical study of liquids should be the basis of the study of life is not without some measure of justification. Most of the osmotic growths produced by Leduc are of aquatic character but we know with tolerable certainty that aquatic life was the first that appeared upon the earth. In living things the crystalloids, electrolysable and non-electrolysable, as well as the colloids act, Leduc declares, in regard to diffusion in them, after the manner of osmotic membranes and the study of their action upon each other seems capable of revealing much that is still unknown concerning the processes employed by nature in the production of life. In some instances diffusion is shown by this writer as well as by Lebon to display the centres and fields of force exhibited in the well-known electrical experiments of Faraday with the same phenomena of attraction and repulsion. In addition to this, there are examples of periodicity in the interactions of liquids which tend to show analogy with the rhythmical characteristics observable in life and lend a certain amount of support to the contention that in its early stages, after the globe had emerged from the anhydrous state of its origin, the calcium salts, carbonates, phosphates and silicates which enter so largely into the composition of living things become organised under the influence of osmotic pressure. It must be remarked, however, that if this were true, there would seem to be no reason why the same phenomenon should not be produced in the laboratory to-day. Of course if spontaneous generation could ever be proved to occur beyond the slightest doubt in the case of micro-organisms, then biology would have to discover the substances, combinations of substances and the special conditions which would enable an advance to be made in its reconstructions to higher forms of life. But this, even if in germinality of inorganic matter and in osmotic pressure

we possessed the keys of life and form, would be a task of the greatest magnitude. For each stage of development, a fresh synthesis might be needed, unless indeed the faculty of reproduction were acquired by the synthetic organisms and it were found possible to make use of it in such a way and on such a scale as to curtail enormously the stages used by nature in the process of evolution and thus to pass from form to form. We should then know whether one original protoplasmic substance or combination of substances possessed the reproductive quality potentially or whether that quality belonged to a plurality of such substances. This, however, is but speculation.

Whatever it may have been at its origin, the experiments of Davenport and others have shown that protoplasm of different organisms now varies in character, not only from species to species but even in the same species according to locality, since different chemical agents act differently upon it. There is a probability, therefore, that if reproductive life were formed in the laboratory it would not proceed from one chemical formula but from many. If germination from the inorganic really happened at an early stage of the world's history, it is difficult to deny that the chemistry of the earth's crust, varying from place to place according to geological formation and to thermal and other conditions, must have produced protoplasmic substances of different constituents with different morphogenetic characters and limitations. As Duclaux has pointed out, however, a micro-organism formed by chemical synthesis as now practised can only disappear, eventually, because in every chemical interaction, properly understood, the interacting bodies are destroyed to form new combinations or if the organism did not disappear, as it would not if the new combinations were insoluble, it would reach a static condition as soon as action was complete. So that if the inorganic originally became germinative it would seem that it must have been endowed with an attribute of continuity which it is not now known to possess. But leaving that aside, it appears probable, for the reasons given above, that if life sprang from the earth's material, it must have sprung from that material in a plurality of forms, from each of which reproduction took place and ultimately what is known as variation. Under this hypothesis the forms of life now extant which vary so greatly according to latitude would have been derived, not each from a

separate formula but from a restricted number of formulæ, from which differentiation has occurred. This would follow from the fact that the known inorganic constituents of life are not very numerous and it would undoubtedly have been from them that nature drew in the original synthesis. If the germinative properties of the inorganic were proved, we should have to regard the elements mainly concerned in the constitution of living substance as life-elements or rather as those from which the reconstructive combinations must be made. If it were possible to analyse living protoplasm chemically, we should probably possess a key to the synthetic reconstruction with these materials and with others which the analysis might reveal. But the means have not yet been found for doing this. The agents used in chemical analysis kill life and until a new method of investigation is devised the ignorance in which we are at present must continue. Living protoplasm, moreover, is not a static substance. It is in constant process of change. It varies qualitatively and quantitatively at every instant of existence. But if the original protoplasm or protoplasms were an effect of terrestrial chemistry and did not descend to us from another sphere, as one hypothesis supposed, we might perhaps expect that the earth should show at least some sign of this reproductive power. We might anticipate that the cooled earth's crust should produce some constantly evolving, semi-vital substance different from ordinary matter. That substance possibly is radium which the earth is constantly producing and which may not inconceivably contain germinative properties in itself. For this substance not only "lives and dies" but while active possesses extraordinary powers of diffusion and of penetration. It is possible that more elements may yet be discovered which will vary still more from the previously known constituents of the planet.

Even if it were proved that the objects hitherto produced in the laboratory were due to effects of crystallisation or fermentation of organic materials and that their reproductivity was thus to be accounted for, it is still certain that living things are constituted out of the same matter as the earth and that therefore it is not illegitimate to seek to reproduce them from that matter.

If some attempts to do so fail, that is no argument that all are

destined to be sterile. There is still reason to hope that the required synthesis will sooner or later be achieved. Life is probably a resultant of a number of forces—osmosis, diffusion, accretion, nutrition and assimilation, heat and electricity acting, it may be, upon certain salts ; the task of biology is to discover how these forces act and the exact nature of the substances on which they act. It would seem that this knowledge should be obtained before the laws of heredity can be established upon any but a risky basis, for as long as the process of life remains undiscovered and the chemistry of living protoplasm is unknown, it is impossible to arrive at any exact determination of the part played by the reproductive cells in the transmission of character from parent to offspring. Some useful information may, no doubt, be obtained by a careful study of the effects of gametic action but there will always be disconcerting exceptions to any general rules that are established on these effects in ignorance of the causes which produce them. Moreover, if living protoplasm varies in its chemical composition from organism to organism, general principles of heredity would not hold good. If the exact composition of the gamete is not known for each variety of plant and animal, it is impossible to pre-ascertain the effects of the geographically varying environment upon it.

If we do not know the exact effect of food substances upon the growing protoplasm, we are again unable to account for departures from a normality established on a far too wide generalisation. For these reasons the laws of heredity cannot be definitely established until the analysis of living protoplasm has been performed and until the true nature of its reactions with the outer world has been determined.

This is the work of biology. The period of surmise and generalisation from incomplete data is over. The biologists must look to chemistry and to physics for the explanation of the life phenomenon. Fortunately the laboratories are active and we may hope for great elucidations in a not far distant future. That life originally sprang from the inanimate is hardly doubtful. That there is no fundamental difference between the inorganic and the organic is proved by the fact that a large number of substances long held to be unreproducible in the laboratory have been there reproduced. It seems that the inanimate must, if there were derivation at all, have given

rise to the animate. At all events the reverse process is almost inconceivable. There are strong reasons to believe, it must be repeated, that the solution of the life problem is to be sought in chemical action and the energies which it develops.

VITALISM

By L. DONCASTER
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One of the most characteristic movements of recent biological thought has been the renewed interest in vitalistic hypotheses. The mechanical conception of living things has presented difficulties unknown thirty years ago and the belief then widely held that the difficulties in the way of a physico-chemical theory of life would rapidly disappear with increased knowledge has not been justified. Hence philosophical biologists like Driesch and J. A. Thomson and biological philosophers like Bergson have fallen back on the belief that life is more than organic chemistry and the physics of colloids and that some extra-physical entity must be imagined if the phenomena are to be explained.

Foremost among modern vitalists is undoubtedly Hans Driesch. His theory of life is so closely interwoven with philosophy that it may seem rash for a mere biologist to discuss his views but biologists of the anti-vitalistic school have done him such scant justice that in fairness some answer from the same quarter is required. It is not difficult, if we take certain features of various vitalistic hypotheses which are not necessarily in agreement among themselves, to find inconsistencies and even absurdities but it is hardly fair to treat in this way a far-reaching and coherent theory like that of Driesch. The vitalism of Driesch rests essentially on a double foundation: the impossibility (according to him) of finding a mechanical explanation, on the one hand, of growth and regeneration, on the other hand, of conscious action. Most critics deal only with the former and for the present we will follow them, taking for the moment as an example a thoughtful discussion of the matter recently published by Dr. Jenkinson.[1] He says quite rightly that Driesch founds the first part of his argument on the fact that any part of the egg of certain animals can give rise to a whole larva with all its parts in proportion. "Each part of the

[1] *Hibbert Journal*, April, 1911.

egg can therefore, according to the needs of the case, give rise to any part of the resulting organism." Now Driesch asserts that although a mechanism might be imagined which will bring about development within an undivided egg, it is impossible to imagine one which can be subdivided at any point and in any direction and yet remain a complete machine and cause any sufficiently large part to develop into a complete organism. It may be that Driesch is mistaken in his denial of this possibility, for a magnet can be divided in various ways and each part will still be a magnet but the answer commonly given is not of this nature and has already been met by Driesch himself. Dr. Jenkinson, for example, writes :

" The reply to this contention is simple. It is not true that the development capacity of every part of the germ is so unrestricted. For experiment has taught us, first, that there are in the unfertilised ovum certain different substances, definitely and necessarily connected with the development of certain organs, since their removal entails the absence of the organ to which each is appropriate ; secondly (and here some of the evidence has been brought forward by Driesch himself), that in the process of segmentation a time always comes when these substances become allocated to the several cells and that thereafter the potentialities of those cells are limited. At what particular moment that limitation occurs depends on the original arrangement of the substances in the ovum, which is variable. Only so long as each cell contains a sample of each substance does it retain the capacity for total development."

All this of course is perfectly true but as Driesch has pointed out, it does not invalidate his argument. In some eggs the " organ-forming " substances are definitely arranged at the time of laying, in others they are more or less uniformly distributed and only become localised during development. But the point of Driesch's contention is that their localisation cannot be the work of a machine contained within the egg, if that machine can be subdivided in any possible manner and yet remain capable of doing its work to perfection. In some cases the work of arrangement is done before the egg is laid, in others only during the early development of the embryo ; but the presence of substances necessary for the formation of certain parts is no more proof of the existence of a machine than the presence of separate piles of bricks, stones, wood, etc., where a house is to be built is a proof that the house is put together

automatically by a machine constructed for that purpose. This might be the case if all we knew was that the removal of the wood caused the house to be built without floors or roof but if we observed that after all the materials were mixed together indiscriminately and then half carted away, a house half the normal size appeared, we should infer that a directing intelligence was present which did the best that was possible with the available materials. The only "machine" conceivable in this latter case would be one consisting in the mutual attractions or affinities of the constituent parts (*cf.* the comparison to a magnet given above) but to account for the development and differentiation of an organism by such hypothesis is to bring into play a whole array of physico-chemical forces which at present are quite unknown and is in fact vitalism ashamed to call itself by its proper name. The fallacy of this kind of criticism of Driesch's hypothesis lies in the assumption that because certain substances are necessary for the development of certain parts, they are the "cause" of that development; beams or girders may be necessary for the production of the floors in a building but they cannot be called their cause; if the beams provided for the purpose were removed while the building was in progress and it was then found that arches of brick were substituted, at least a *primâ facie* case would be made out for a directing intelligence rather than a machine. It is to facts of this order that Driesch appeals in support of his hypothesis and although he perhaps goes too far in saying that the possibility of a machine is excluded, his contention cannot be opposed by proving that certain materials are necessary for certain results, unless it can be shown that those materials are sufficient in themselves to bring the results about. The chemistry and physics of living matter may be able to do this in the future but are far from doing so at present; therefore for the present his position is unassailable in this direction.

The matter can only be furthered by careful observation and well-planned experiment, in which the biologist must work in co-operation with the physicist and chemist. At the present time the student of living things shows a tendency to regard any phenomenon as "explained" when once it falls under known laws of physiological stimulation or metabolism, although the ultimate causes of these things are unknown. It rests with the future to discover whether they have a purely

physical basis; at present the only safe course is to confess ignorance.

A more fundamental objection to vitalism, however, remains. It appears to contradict the principle, on which all science is really founded, that not more ultimate causes must be postulated than are absolutely necessary. Science has proceeded far in recent years in the unification of all the phenomena associated with matter; it appears a retrograde step to assume something fundamentally different in the living from what exists in the non-living. Of course if, with Driesch, we regard it as demonstrable that physico-chemical factors are insufficient to account for the phenomena of life, this objection falls; but it may be worth while for those who do not go so far and who think that he has only made out a good case rather than adduced conclusive proof, to consider whether his hypothesis does contradict the fundamental principle referred to. For this purpose we must consider his idea rather more closely. He finds two characteristics displayed in common by an organism which is developing from less differentiated to a more complex bodily form on the one hand or which is acting more or less consciously on the other.

In each case the process appears to be purposeful, *i.e.* when it is completed we can see that it had an object; and in each case it makes use of matter and of physical and chemical energy in such a way as to promote the apparent "purpose" without in any way contradicting the ordinary laws of matter and energy. Hence he suggests that the controlling principle is similar in each case; his names, "entelechy" for the principle which controls development or regeneration and "psychoid" for that controlling action, are unimportant; the essential thing is that in each case matter and energies are used in such a way that the result is different from what it would have been in the absence of the controlling principle and that to the onlooker the process appears purposeful. The controlling principle is not energy and the energies which it directs are simply those known to chemistry or physics; it is merely assumed that just as a man, when he lifts a weight, converts into kinetic form energies which would have remained potential if he had willed to remain at rest, so an organism developing from an egg or regenerating a limb, sets free or controls energies which in the absence of life would have taken a different course. It has

been maintained that this implies as many distinct controlling principles as there are kinds of organisms or even individuals; but this is surely a mistake. It is equally possible that the material at the disposal of the principle controlling development will determine the form it takes, as the circumstances which surround a man determine his action, without being the final "cause" in either case. A man who wants to build a house will build one of wood in a land where trees are plentiful and stones scarce, yet no one will say that the abundance of trees is the cause of his building the house. So the principle controlling development may cause a star-fish to develop from one kind of ovum and a dog from another, not because the principle is different but because the ovum of each is adapted only to the production of that kind of result.

Further, it should be noticed that one objection often brought against vitalism is not valid against this suggestion, the argument, namely, that many substances formerly believed to be produced only by living organisms have now been synthesised in the laboratory and that others, which now defy the chemist, may be conquered to-morrow. A plant, by its control of certain chemical energies, causes the production of the compound indigo. The chemist analyses the natural compound and proceeds to sythesise it in his laboratory. This, however, is no proof that the compound can ever occur in nature in the absence of life, for the chemist imitates the plant in allowing certain energies to come into play and keeping away others which would interfere with the action if they were present. No "vital energy" is required; if energy is needed from outside he turns on his gas-burner and applies a flame to his test-tube, so obtaining from chemical combination a supply which would have remained potential but for his intervention.

It can hardly be doubted that the recognition of a principle of this kind, which unconsciously, perhaps—or at least not consciously in the sense in which man uses the word of himself—seeks to produce individuality out of incoherence and self-expression through material form would greatly simplify many of the more difficult problems of biology. It is generally rejected by biologists, partly because of the very real danger that if it were recognised every unexplained phenomenon might be ascribed to its action and still more because it seems to conflict with the principle of non-multiplication of entities. But is it

certain that this conflict is a reality? If we begin at the top of the scale instead of at the bottom, we find in ourselves certain faculties which we call mental or spiritual. Only two possibilities seem open to us; either these faculties are the product of Verworn's "chemistry of the proteids" or they have a non-material basis. Many biologists and physiologists, accustomed as they are to think in terms of matter and to analyse the behaviour of organisms into a play of chemical and physical forces, incline to the former alternative. To them any form of vitalism adds a new entity to the universe and is therefore condemned. But those whose training has dealt especially with the mind commonly reject this view, which is after all only a restatement of the older assertion that the brain secretes thought as the liver secretes bile. If these are right in their belief that there is in man an entity which is not reducible to terms of matter and energy, again two alternatives are open. Either this non-material principle has been present in living matter from the first and perhaps before what we call "living" came into existence; or it entered suddenly into living beings when they were ready for it and in fact enters suddenly into each one at some moment of his existence, for it seems absurd to ascribe a "soul" to a human ovum and to deny it to an adult ape or dog. Of these two alternatives again the former seems the only reasonable one, for the day of catastrophism has gone by. If then we agree that mind is not a function of proteid chemistry and if we admit that the relations with matter of mental and instinctive action are characterised by purposive control of energies, the natural form for unification to take is to suppose that the apparently purposeful control of matter and energy seen in organic development is of the same ultimate nature. It has been suggested repeatedly that not only instinct but also the successive steps in the history of a life are to be regarded as governed by something akin to memory. The expressions unconscious or inherited memory, however, are open to grave objection, for it is not certain that they are more than misleading analogies and at best presuppose true memory, the ultimate basis of which is equally obscure. If, however, we admit the existence of something extra-physical in the mental phenomena which we ourselves experience, if we find that some at least of its characteristics are manifested in instincts and growth-processes at present inexplicable on a mechanical scheme, it

seems reasonable at least to allow the possibility that all these things have a common extra-physical source. Such a "vital principle," if existent, would doubtless act in accordance with ascertainable laws; the task of biology would be to investigate them, to trace exactly the limits between the physical and the vital and to discover the effects of their interaction. So long as vitalism is not used to provide a facile explanation of any phenomenon which is not otherwise readily explicable, some such hypothesis may serve as a starting-point for further inquiry and may give hope of providing a common basis for phenomena as far apart as the segmentation of a sea-urchin's egg and the mental and spiritual faculties of man.

AERATION OF THE SOIL BY EARTH-WORMS

By J. NEWTON FRIEND, Ph.D., D.Sc.

During the past few years so much attention has been given to the numerous problems connected with the corrosion of iron and steel that chemists are now beginning to realise that a large number of hitherto unsuspected factors are involved in the change. Richard Gaines (*Chemical News*, 1910, **101**, 205) has brought forward a body of evidence to show that bacteria not infrequently play an important part in the oxidation of steel structures embedded in the soil and it seemed to me that the common earthworm might likewise prove to be a potent factor in similar cases. It is now well recognised that water, acids and oxygen are essential to corrosion and it may be expected that if the amounts of these three substances in the soil be increased corrosion will be stimulated. It would appear that worms are capable of assisting the distribution of moisture in the soil both by continually burrowing in it and by swallowing and then excreting it. In so far as this is the case, therefore, they may assist corrosion, even although they may not add to the total amount of water in the soil.

But the chief activity of worms lies in their power of excreting acids in their castings, of exhaling carbon dioxide and of aerating the soil both by mechanical and by chemical means; it was with the object of determining the potency of these factors that the observations now recorded were made.

A search through the available current zoological literature shows how lamentably ignorant we are of the habits and distribution of worms. This is further emphasised by the fact that although reasonably cheap text-books may be purchased on practically all the usual natural history topics, we have no corresponding text-book on worms. It is hoped that the information contained in this paper may serve to arouse interest afresh in the earthworm; as we shall see, it has a wonderful work to perform.

The Worms Studied.—In a recent communication to the *Zoologist* (April 1911, p. 142) my father, the Rev. Hilderic Friend, has given a complete list of the British Annelids and the system of nomenclature he there uses is adopted here. The worms studied by me were as follows:

1. *Lumbricus rubellus* (Hoffm.). The Red worm, from Norfolk Broads.
2. *Allolobophora longa* (Ude). The Long worm, from Swadlincote, Burton-on-Trent.
3. *Aporrectodea chlorotica* (Sav.). The Green worm, from clayey soil in Darlington.
4. *Octolasium cyaneum* (Sav.). From Swadlincote.
5. *Dendrobaena*. Species uncertain. This genus includes the smallest British Annelids. From Swadlincote.

Nos. 1, 2, 4 and 5 were kindly identified and forwarded to me by my father, whom I take this opportunity of thanking.

Relative Density of Worms.—If worms are thrown into water, they invariably sink, showing that, though not possessed of bones, etc., their density is greater than unity. It was of interest to determine the densities of a few species of worms so that, assuming the value to remain constant for all the Annelids, it might be easy to calculate the volumes of smaller worms whose weights can easily be ascertained but whose volumes are too small to admit of accurate measurement. The worms were washed in gently flowing water, dried as satisfactorily as possible with filter papers and introduced into burettes containing known volumes of water. The difference in volume thus produced could easily be read to 0·05 cubic centimetres, so that the volumes were usually accurate to about 1 per cent. The results obtained are given in the following table:

The Densities of Worms

Species.	Number of worms taken.	Weight of worms.	Volume of worms.	Relative density.
		Grammes.	C.C.	
A. longa . . .	3	9·08	8·0	1·13
,, ...	7	20·63	18·5	1·12
,, ...	10	24·47	22·7	1·08
O. cyaneum . . .	10	11·94	11·1	1·08
A. chlorotica . . .	10	2·15	1·9	1·13

From this it is clear that the average relative density of the Annelids, regarding the specimens tested as fair types, is 1·10.

Accepting this figure, we only have to weigh worms, divide the result by 1·10 and the volume is obtained. This may be very useful when investigating small species of which only a single specimen or at best but few specimens can be obtained.

The Ratio of the Diameter to the Length of Worms.—This ratio is very difficult to obtain accurately, as worms are capable of considerable extension. In order to determine the length, l, the worms were sprinkled with sand to allow of obtaining a good grip, then gently drawn out until a definite point of resistance was attained. The length was noted. The worms did not appear to suffer in the slightest from this treatment. The volumes of the worms were determined by immersing in water in a burette as before and the diameters calculated from the expression

$$d = \sqrt{\frac{4V}{\pi l}}$$

where d = diameter, l = length, V = volume and $\pi = 3{\cdot}1416$. This assumes that the worm is truly cylindrical, which it is not, since it tapers at the head and tail; consequently the results are only approximate but the error will be much the same for all the worms.

Only the larger worms could be treated in this way, as in the case of the small ones any slight error in the determination of the length would produce a proportionately large error in the calculation of the diameter. If the worms could be tested when dead the length could be determined easily enough for all worms, large or small; but if killed in spirit their bodies harden and render the measurement impossible. If killed in other ways, the delicate skin of the worms is apt to give way under the stretching and to yield uncertain results.

In a series of experiments with ten specimens of *A. longa*, the average lengths of which were 20·2 cm. and average diameters 0·378 cm., the ratio:

$$\frac{\text{diameter}}{\text{length}} = \frac{d}{l} = \frac{0{\cdot}378}{20{\cdot}2} = \frac{1}{53}.$$

In a similar manner ten specimens of *O. cyaneum* gave the ratio:

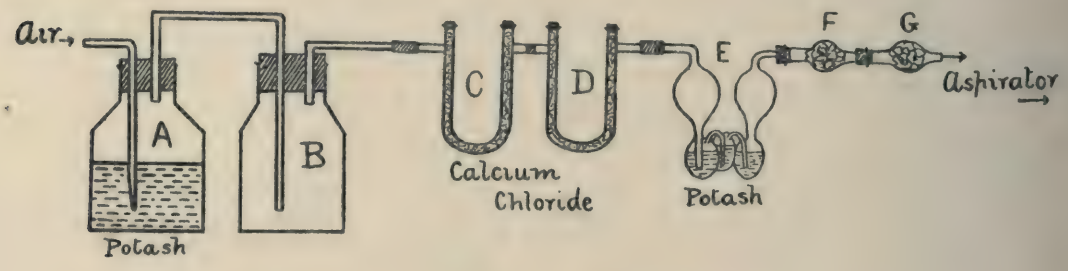
$$\frac{d}{l} = \frac{0{\cdot}264}{14{\cdot}9} = \frac{1}{52}.$$

Whether or not this close agreement is due to coincidence cannot be definitely decided until further experiments have

been carried out but the result is sufficiently striking to merit attention.

The assumption that the ratio d/l may be constant for the Annelids is by no means so unreasonable as might appear at first sight. The structure of the worms is extremely rudimentary, the major portion of their length consisting of digestive organs and intestines of the simplest type. Consequently the longer the worm the longer is the digestive tract and the greater the amount of food that can be dealt with; hence the bigger the worm. This, of course, is no proof that d/l is constant but it shows that such might easily be the case, as the above results indicate. Individual specimens of the same species may be expected to vary just a little, as also the different species themselves, from one another but for approximate calculations these mathematical relationships may prove very useful. Thus, granted that the ratio of length to diameter is constant, by determining the weight (in grams) of any worm, we can calculate its volume, diameter, length and surface area—factors of interest from agricultural and horticultural points of view.

Carbon Dioxide excreted by Worms.—In order to determine the weight of carbon dioxide excreted by worms the apparatus shown in the figure was fitted up. The worms were



placed in the flask B; in the case of the larger species this contained glass stoppers so that the worms could crawl about more easily. Air freed from carbon dioxide by passage through the potash in A was slowly aspirated through the apparatus, being dried by the U-tubes C and D which contained calcium chloride. The bulbs E contained caustic potash which absorbed any carbon dioxide breathed into the air by the worms. By weighing E before and after each experiment the weight of this carbon dioxide was determined. F and G are two guard drying tubes, F being weighed with E in case any moisture should

have evaporated into it from the potash in E. G served to prevent any moisture from the aspirator from striking back into the apparatus and affecting the results. Sometimes B was covered with a box so that the worms were kept in complete darkness.

The results obtained are given in the following table; the last column of figures in the table is obtained by dividing the weight of carbon dioxide exhaled in twenty-four hours by the weight of the worms tested. For convenience we shall call this the carbon dioxide factor.

CARBON DIOXIDE BREATHED OUT BY WORMS

Experiment No.	Species of worm.	Total weight of worms.	No. of worms.	Average weight of each worm.	Duration of experiment.	Weight of carbon dioxide exhaled.	Carbon dioxide factor.	Remarks.
		Grams.		Grams.	Hours.	Grams.		
1	*L. rubellus*	8·59	12	0·716	5	0·0088	1/204	—
2	—	—	—	—	—	—	—	—
3	*L. rubellus*	8·59	12	0·716	43·5	0·0852	1/183	—
4	,,	2·86	4	0·715	24	0·0142	1/200	—
5	,,	2·86	4	0·715	24	0·0082	1/349	Worms were dying
6	*A. longa*	15·80	5	3·16	9	0·0112	1/529	Daylight
7	,,	7·35	3	2·45	23	0·0131	1/538	Covered part of time
8	,,	8·28	3	2·76	18	0·0101	1/615	Covered
9	,,	10·96	3	3·65	9	0·0076	1/541	Daylight
10	,,	10·96	3	3·65	28	0·0116	1/1102	Covered
11	,,	10·96	3	3·65	11	0·0066	1/761	Daylight
12	,,	9·08	3	3·03	8·75	0·0069	1/479	Daylight
13	,,	9·08	3	3·03	15	0·0090	1/631	Covered
14	,,	9·08	3	3·03	9	0·0044	1/774	Daylight
15	*A. chlorotica*	2·15	10	0·215	8	0·0038	1/189	Daylight
16	,,	2·15	10	0·215	15	0·0073	1/184	Covered
17	,,	1·71	11	0·156	14·5	0·0040	1/258	Covered
18	,,	1·71	11	0·156	8·75	0·0028	1/222	Covered
19	,,	1·71	11	0·156	15·75	0·0049	1/229	Covered
20	*O. cyaneum*	11·94	10	0·1194	23·5	0·0155[1]	1/754	Covered
21	*Dendrobaena*	0·484	7	0·069	23·5	0·0041	1/115	Covered

From the above results a number of very interesting facts may be gleaned. These may be summarised as follows:

1. The amount of carbon dioxide exhaled is not proportional to the body weight of the worms, the smaller species exhaling larger quantities of the gas than the larger in proportion to

[1] This result is probably a little low. The worms were dying towards the end of the experiment.

their weight; in other words the carbon dioxide factor rises as the worms decrease in size.

Experiment shows that the average human adult under normal conditions exhales some 746 grams of carbon dioxide in twenty-four hours. Assuming his weight to be 9 stone or 453·6 × 126 grams (1 lb. = 453·6 grams) it is easy to see that the carbon dioxide factor is 746/453·6 × 126 = 1/76.

If the person is sleeping or unusually quiet this factor may fall to 1/100 and lower. Hence, the carbon dioxide factor of the smallest Annelids, as represented by *Dendrobaena* in the table above, is practically the same as that of human beings in a similarly quiescent state. This is a result that one could hardly have expected from *a priori* reasoning.

2. The carbon dioxide factor depends upon the temperature. This may be concluded from the results of experiments 17, 18 and 19 with *A. chlorotica*. The same worms were used in the tests, being dug out of the ground on November 9, 1911, when the air was very cold although not actually freezing at the time. The worms were almost immediately put into the apparatus in the warm laboratory at 15° C. They took some time to regain full activity, consequently the factor is low in experiment 17 but rises again in Nos. 18 and 19. This result is precisely what we might expect, as during the winter annelid vitality is always low in common with that of many of the higher forms of life.

3. Under the influence of daylight the carbon dioxide factor shows a slight but decided increase. This is evident from experiments 6 to 14. Nos. 6, 7 and 8 are strictly comparable with one another, as each one was carried out with fresh worms which, therefore, were not fatigued through absence of food. In experiments 9, 10 and 11 the same worms were used continuously and the fall of the carbon dioxide factor from 1/541 in No. 9 to 1/761 in No. 11 indicates the rate of exhaustion during experimentation. The mean of these results, namely 1/630, is appreciably higher than the result for experiment 10. Similarly the mean of experiments 12 and 14 is 1/592, which is higher than the result in experiment 13.

In experiments 15 and 16 the carbon dioxide factor is higher in the case of the worms exposed to darkness. The difference, however, is well within the error of experiment and serves to show either that all worms are not equally sensitive to the action of light or else that there are times when the worms are

less sensitive to external stimulus than usual. This latter conclusion is in accordance with the observations of Darwin (see *Vegetable Mould and Earthworms*, by Darwin, Chap. I.) who performed some very interesting experiments on worms, undoubtedly Annelids but of undetermined species. In order to verify the statement made by Hoffmeister in 1845 that worms are sensitive to the action of light, Darwin exposed some worms, kept in pots, to candle light and observed that usually they were not affected at first. Sometimes, however, they instantly withdrew into their burrows as soon as the light fell on them. This occurred perhaps once out of a dozen times. But all the worms were influenced by the light if exposed to the latter for a sufficient length of time. Darwin's experiments were purely qualitative but apparently receive indirect confirmation from my own quantitative experiments detailed above.

The Acidity of Worm Castings.—No experiments have been made to determine quantitatively the acidity of worm castings, as this is a subject fraught with peculiar difficulty in view of the fact that the acidity is extremely slight and is readily lost. Thus Darwin mentions that the castings examined by himself, even though clearly acid when first ejected, were perfectly neutral on the succeeding morning. In chalky soil the castings are usually free from all traces of free acid.

Worms as Aerators of the Soil.—The worms effect the aeration of the soil in a variety of ways, the more important of which may be enumerated as follows :

1. By burrowing through the soil the worms render the latter more porous and permeable to gases, not merely by virtue of the air spaces formed but by reason of the fact that the soil is thus continually kept in gentle motion.

2. The soil passing through the bodies of worms is excreted in a finer condition, being ground by attrition within the intestines. Now increased fineness up to a certain limit means increased area and hence greater susceptibility to the action of gases ; beyond this limit the particles begin to cling together and we get a proportionately less porous mass more resembling clay than ordinary soil. Darwin estimates that no fewer than fifteen tons of soil annually pass through the bodies of worms for every acre.

3. The worms breathe in oxygen and exhale carbon dioxide, which latter, being readily soluble, largely dissolves in water in

the soil, so that more oxygen must diffuse towards the worms from the air above to take its place. Thus a constant interchange of gases is maintained.

Assuming with Darwin that there are some 27,000 worms existing per acre of soil, it should be possible to calculate the rate of interchange of gases thus set up, provided we know the relative distribution of the different species present. Unfortunately this is not known but taking the mean of the large and small species the average worm may be assumed to weigh 0·6 gram and to have a carbon dioxide factor of 1/200. Each worm will then exhale 0·003 gram of carbon dioxide in twenty-four hours; 27,000 worms will exhale 81 grams or approximately 40 litres of the gas. In the course of a year this would amount to 29·6 kilograms or 15,000 litres of carbon dioxide per acre. Since for every litre of gas evolved an equal volume of oxygen must find its way into the soil from the air, it is clear that for the whole of Great Britain, which contains many millions of acres, the total gaseous interchange due to this cause alone must be very considerable.

Incidentally it is interesting to note that the carbon dioxide exhaled per acre during a whole year is sufficient under favourable conditions to effect the solution of 37·7 kilograms (83 lb.) of metallic iron. This amount is certainly not negligible, particularly when we remember that during corrosion, when the dissolved metal is thrown out of solution as rust, the acid is liberated and is thus rendered free to attack more iron. Furthermore, Darwin states that some worms of his were kept in test pots filled with sand of a red colour, due to the presence of anhydrous ferric oxide—a slightly soluble body and one from which we can imagine plants are unable to assimilate the iron directly. It was observed, however, that the burrows made by the worms were lined with their castings, as usual, composed of *colourless* sand and intestinal secretions. Examination under the microscope proved that the ferric oxide had been completely dissolved out of the sand by the digestive action of the worms, although it was of such an insoluble nature that the usual mineral acids diluted according to the British Pharmacopeia produced less effect. It is to be presumed that this iron either entered into the composition of the body of the worm itself or, more likely, was excreted in some soluble form. In either case the iron must eventually be given back to the soil in a more

soluble condition, presumably in one which can be directly absorbed by the plant roots.

This opens up another sphere of usefulness in which the humble worm does great service and one to which, so far as I am aware, no one has hitherto drawn attention. I wish therefore to thank my colleague, Mr. R. H. Sargent, for suggesting its possibility to me.

From the above considerations it is obvious that worms have a very important ministry to perform in the soil and it is hoped that naturalists and others may be stimulated to extend their observations into this practically untrodden field, so that the next decade may witness a rapid increase in our knowledge of the soil and its inhabitants.

MATHEMATICS AND CHEMISTRY

By F. P. WORLEY, M.A.

"The tendency to hide ignorance under the cover of a mathematical formula had already appeared but was not openly advocated; hence students were still taught to form definite ideas of the processes of nature."—ARTHUR SCHUSTER, *The Progress of Physics*, 1875–1908.

THE publication of a new book on Mathematics for Chemists[1] may well be regarded as an opportunity for a short discussion of the relation between chemistry and mathematics from the point of view of the many students who nowadays are called upon to use mathematics in the investigation of chemical problems. To the undergraduate, struggling against various unnecessary and unnatural obstacles to obtain a degree, such a book has two uses—he may derive from it sufficient superficial knowledge to satisfy his examiners; it also may enable him to understand the mathematical part of the works he has to read and master.

But unfortunately, under our present educational system, the undergraduate is little concerned with the use and application of mathematics as a means of investigating actual problems: not only is he not required to carry out anything in the nature of an original investigation but more often than not he is discouraged, if not practically prevented, from so doing. Still, though we are not here concerned with the utility of a book for examination purposes, this being a matter more for writers, publishers and examination coaches, we cannot but remember that such utility is the factor which chiefly determines the demand for a book and can only regret that it is unfortunately achieved very much at the expense of strictly scientific value.

Chemists, as a rule, know very little mathematics but even when they have received what is considered to be a fair amount of mathematical training they only too frequently find that their knowledge is not sufficient to enable them to deal with the

[1] *Higher Mathematics for Chemical Students.* By J. Riddick Partington, B.Sc., pp. 272 and 44 diagrams. Methuen & Co., London, 1911. 5*s*.

practical problems that arise; unfortunately, they also too often find that the mathematician has not sufficient chemical knowledge and feeling to give them the assistance they need. Speaking on behalf of the mathematician, on the other hand, chemists rarely express their problems properly. They never seem quite clear as to what is to be assumed as known and exactly what is to be proved. Whilst one realises that a mathematical training that would enable and induce the chemist to express his ideas and problems with clear mathematical precision would be of inestimable value, still it must be admitted that chemical problems are frequently of such a nature that it is impossible to be absolutely certain of anything; the chemist frequently does *not* know what he wants to prove nor indeed does he want to *prove* anything; he wants merely to put a reasonable interpretation upon certain experimental results.

A still more important question to consider is whether, except in rare cases, the chemical and mathematical habits of mind are compatible—chemical properties are the expression of the reciprocal behaviour of substances, not absolute quantities; on this account it is very difficult to quantify such properties: often they can be felt but not figured.

Before dealing with the mathematical requirements of the chemist for the purposes of investigation and research, it is well perhaps to emphasise the great need of giving all chemists a sufficient mathematical training—not much is needed but it must be sound—to enable them to read intelligently and critically the numerous mathematical papers that are continually appearing in chemical journals. To the chemist, such papers, as a rule, are uninviting and often produce a feeling of mistrust and suspicion, especially when, by means that he cannot follow, conclusions are arrived at which are not in harmony with his own feelings. Investigation often shows that these feelings of mistrust and suspicion are unfortunately only too well founded; there can be no doubt, in fact, that a large proportion of the published mathematical discussions of chemical problems is in need of serious reconsideration.

The chemist often feels that if only he had the necessary mathematical insight, he would find that the cooking-pot had a paper bottom. As it is his vision cannot penetrate the stew and he perceives only the oily iridescence on the surface or the reflection of nature above. I do not wish to imply that the

results have been "cooked" or "faked," to use terms familiar to many students and teachers of chemistry. The work is undoubtedly thoroughly conscientious but either the problem has been incorrectly stated or the conclusions have been unconsciously forced to fit the preconceived ideas of the investigator. There is a fascination in arriving at a mathematical "proof" of an hypothesis and especially in obtaining a good "constant" which makes it imperative that chemists should be taught to be most cautious and critical when their results seem to confirm their anticipations. We are apt to overlook the fact that a number of different equations may often fit a given set of facts equally well and that a variety of assumptions may lead to just as good a constant.

One example taken from the *Zeitschrift für physikalische Chemie* may help to show how easily errors are made and overlooked. It is from a paper by A. A. Noyes and J. Seidensticker which confirmed (?) Jakowkin's conclusions that in solutions of iodine and potassium iodide in water the iodine combined with the potassium iodide to form the tri-iodide in the manner indicated by the expression: $KI + I_2 = KI_3$. They determined the solubility of iodine in aqueous solutions of potassium iodide of different concentrations and assumed that the concentration of the free iodine was the same as in water alone at the same temperature and that the excess of iodine over that dissolved by water was combined with the potassium iodide in the form of KI_3. By deducting the solubility of iodine in water, they obtained the concentration of the iodine combined as KI_3 for each concentration of potassium iodide—one molecule of iodine, I_2, forming one of KI_3. As the concentration of the uncombined iodine was by assumption constant, the mass action equation for equilibrium, viz.:

$$(KI_3) = k\,(\text{free KI})\,(I_2),$$

simplified to:

$$(KI_3) = k^1\,(\text{free KI}),$$

where the brackets represent the molecular proportions of the respective substances. It followed that (KI_3) should be proportional to $(KI_3)+(\text{free KI})$, that is to say, to the original concentration of the potassium iodide. They found that this ratio was, in fact, approximately constant, as the following table shows; hence they concluded that their assumption was confirmed.

Millimols. KI per litre (free KI + KI_3).	Millimols. I_2 dissolved per litre.	I_2 combined as complex salt (KI_3).	(KI_3) : (KI + KI_3).
106·3	55·28	53·94	0·5074
53·15	28·03	26·69	0·5021
26·57	14·68	13·34	0·5021
13·29	8·003	6·661	0·5012
6·643	4·667	3·325	0·5005
3·322	3·052	1·710	0·5148
1·611	2·235	0·893	0·5376
0·834	1·814	0·472	0·5684
0·00	1·342	—	—

Examination shows, however, that any assumption as to the number of molecules of iodine I_2 combined with one of potassium iodide KI leads to exactly as good a constant. By assuming, for instance, that the compound KI_5 is formed, the values in this column are halved and consequently those in the fourth also, the constancy of the ratio remaining unaffected. Though their investigation showed that the concentration of the combined iodine was proportional to that of the potassium iodide, it proved nothing with regard to its state of combination. It merely showed that at that temperature, for every additional two molecules of iodide KI, an additional molecule of iodine I_2 was dissolved.

It is this abuse of mathematics in discussions on chemical theory that has done and is still doing so much harm to the advancement of chemical science. We still have "proofs" which are proofs only on a host of doubtful assumptions, as well as frequent omissions. Mathematics is too often used in such a way as to obscure the truth by the unnecessary complication of simple interactions or by imposing a spurious simplicity on changes which are undoubtedly more complex than is assumed. The errors are not, as a rule, strictly speaking mathematical but errors in the *application* of mathematics. They are due to defective reasoning with the defects plastered over and obscured by a thick coating of mathematics; remove this plaster and we find probably that some of the facts are missing. We are reminded of the puzzles in which a number of blocks of different shapes are to be arranged into a square, using all the blocks. It is easy enough to form a square by omitting one or two pieces, perhaps insignificantly small ones; but this is a wrong solution, generally fundamentally wrong. So it is with

chemistry. Take, for instance, the vast amount of mathematical juggling that has been done to make the processes of esterification and hydrolysis fit the theory of ionic dissociation of electrolytes in solution. The unruly and unconformable fact that the molecular catalytic activity of acids decreases with aqueous dilution, while the degree of ionic dissociation must be assumed to increase, has been entirely neglected, though discovered by Ostwald as long ago as 1883 and since shown to be true by other observers. To fit this little block into the foundation we may have to raze to the ground all the elaborate superstructures erected by Stieglitz, Acree and Johnson, Goldschmidt and others.

Frequently the errors are more mathematical and the sooner chemists understand clearly the principles and methods of mathematics and can be sufficiently critical, so that they cease treating a mathematical formula as a sort of fetish, the sooner shall we be able to relegate to its proper place—the waste-paper basket—a very large part of the mass of mathematical investigations of chemical problems foisted upon a credulous public of late years. What is needed is a sort of cupellation process for chemical mathematics which will remove the lead and leave the little silver that is in the ore.

With regard to the actual requirements of the chemist in the matter of mathematical training, a few personal opinions based on considerable experience may serve to induce discussion or criticism. What is most essential is not so much the skill of deducing and using mathematical formulæ and of carrying out certain mathematical processes (which a chemist can, if necessary, get a mathematician to do for him) as a thorough drilling in the principles, methods and application of mathematics. This should form a most useful supplement to his chemical training and endow him with a power of reasoning which is definite and exact—in comparison with which his chemical mode of thought is vague and incomplete, dealing as it so often must with problems that are highly and obscurely complex and which cannot at present be subjected to exact mathematical treatment. Without this training, all the mathematical agility a chemist may acquire is likely to be worse than useless. But this is just where not only books but the general attitude of physical chemists towards mathematics seems deficient. Chemists are

taught mathematics without sufficient instruction in the way in which the weapons put into their hands are to be used and especially the way in which they are not to be used.

In this connection it may be questioned whether it is advisable to use chemical problems in the teaching of mathematical processes. They are not necessary for the mathematics and cannot be the real problems of practice but must be spoiled of their difficulties and given a spurious simplicity likely to produce a very false impression. Mr. Partington's book has a wealth of chemical illustrations and useful examples but seems to stop short where the real difficulties begin; it gives the impression that chemical problems are very much simpler and straightforward than is really the case. It would probably be vastly more satisfactory if the necessary parts of mathematics were taught without attempting to deal with chemical problems, with sufficient examples and exercises to make the student proficient in the carrying out of the various processes and if afterwards real chemical problems were dealt with thoroughly. This course would give an insight into the nature of the difficulties actually met with and dispel the idea that the acquirement and use of a few mathematical processes and formulæ will unravel the tangled knots of chemical theory. Such a book, besides teaching the student how to deal with such problems, would also form a most valuable reference for cases not usually dealt with in books on mathematics and physical chemistry. Chemists as a class are never likely to be mathematicians. The very nature of their work and their habit of thought are opposed to the mathematical frame of mind; furthermore, they are never likely to have sufficient practice in mathematics to enable them to rely on their own skill in carrying out even a comparatively simple integration.[1] It is consequently highly desirable that the mathematical treatment of a question, such, for instance, as that of mass action, should be thorough, dealing with all the difficulties that arise. Usually we find it very incompletely dealt with, from a practical point of view, in books on physical

[1] Mistakes are made not only by students but also by more experienced workers. In Mr. Partington's book we find, for instance, on the top of p. 145 a slip that should not be made in a mathematical book, the value of K in the unimolecular mass action equation being given as

$$\frac{1}{t_2 - t_1} \log \frac{a - x_2}{a - x_1} \text{ instead of } \frac{1}{t_2 - t_1} \log \frac{a - x_1}{a - x_2}.$$

chemistry, whilst in books on mathematics the subject affords examples of methods of integration and is distributed according to the methods required for the different cases. We have only to look at the published work of many well-known chemists to realise how much we are in need of sound teaching in connection with this subject of mass action and to see what extraordinary ideas exist as to what constitutes a constant and as to the importance that can be attached to obtaining constants. To the most credulous, it must seem ridiculous that the attempt should be made to base any serious deductions on the results obtained by applying the law of mass action to the effects produced by the action of a poisonous solution on a frog's leg, for example.

In the first place, the student should have it impressed on him that the mass action law is a generalisation of an axiomatic nature, never *apparently* obeyed exactly and incapable therefore of absolute proof; that even if the correct assumptions are made with regard to the number and nature of the interacting molecules there are many other disturbing factors, as a rule, that cannot be taken into account. Then with regard to the deduction of the mathematical formulæ for the investigation of the various cases, not only should all possible simple cases be dealt with but the merits and defects of alternative formulæ should be pointed out. Writers of books on Physical Chemistry, for instance, James Walker and Senter, who presumably know the relative merits of the two formulæ

$$K = \frac{1}{t} \log \frac{a}{a - x} \text{ and } K = \frac{1}{t_2 - t_1} \log \frac{a - x_1}{a - x_2}$$

for the investigation of a unimolecular change, for some reason or other give only the former without even hinting to the poor student that they have passed off on to him a second-rate and untrustworthy article; that his values of K will all be affected by an error in the one value a; that near the beginning of the experiment when a is determined the chance of error due to the temperature not having become constant is greatest; and that the use of this formula does not allow of a strict comparison of different stages of the interaction. By using the second formula, which is immediately deducible from the first, the possibility of an error in all the values of K due to an error in the one value a is obviated; if the values of K show no tendency to rise or fall but remain constant within the limits of experimental error this

error is practically eliminated in the mean value, whilst if the velocity coefficient tends to increase or diminish as the interaction proceeds, the rise or fall of K is rendered immediately evident and we can compare different stages of the interaction. This equation will not give such a smooth *looking* series of values of K as the other but is vastly more trustworthy and scientific.

Besides the straightforward cases of mass action, other difficulties are always being met with. Such cases as the following are so frequent in practice that they should be dealt with. Methylic acetate in dilute aqueous solution is almost completely hydrolysed, at a rate depending on the temperature and the nature and concentration of the catalyst, giving rise to methylic alcohol and acetic acid. The student learns from a text-book that the interaction under these conditions may be treated as unimolecular. He treats it so, assuming that the amount of acetate remaining when equilibrium is attained is negligible and carries out a number of experiments. He finds on later investigation, however, that only 93 or 94 per cent. of the acetate is hydrolysed at equilibrium. What is he to do? Are the values of the velocity coefficient he has obtained by considering only the convertible acetate useless? He cannot simply make fresh calculations from his experiments as he has determined only the concentration of the convertible acetate and not the total; it is very much more difficult, perhaps, to determine the total amount present. The following problem then remains for solution: Granted that the velocity coefficient of hydrolysis is constant when the complete change of simultaneous hydrolysis and esterification is considered, is it to be expected that by considering only the hydrolysis and the convertible acetate the values of the velocity coefficient should also be constant? If so, what is the relation between this value of K and the true value? Another familiar example of the same nature is the conversion of ammonic cyanate into urea studied by James Walker. Such an example presents the difficulties actually experienced and would form a valuable addition to a text-book, especially if it were pointed out what assumptions had been made by the investigator and that others might lead to different conclusions.

Again, suppose that on the application of the mass action law a constant is obtained. Is this a confirmation of the hypothesis used? And if a constant be *not* obtained on any simple assumption, what is to be done? We may determine the order of the

interaction mathematically and find that it is between 2 and 3, say 2·4—but we are no better off. An interaction of an order of 2·4 is meaningless. The order may turn out to be 4—but again, does this mean that the interaction is really one of the fourth order? In practice we find that we seldom have the ideal cases of the text-book, which have been picked out on account of their apparently straightforward nature. Even when a change appears simple and straightforward, as in the case of the hydrolysis of cane sugar examined by the polarimetric method, the constancy of the velocity coefficient may be to a great extent an accident, due to the balance of opposing effects of the products of change on the rate of hydrolysis.

Far too little importance is attached, as a rule, to the value of graphic methods in dealing with problems of mass action. The complete mass action equation for the hydrolysis of an ethereal salt at any one temperature with constant concentration of catalyst is

$$\frac{dx}{dt} = K_1 (a - x)(b - x) - k_2 (c + x)(d + x),$$

where a, b, c, d are the initial molecular concentrations of ethereal salt, water, alcohol and acid and x is the amount of change after time t. On integration this gives an ugly, great equation,

$$k_2 = \frac{1}{t} \cdot \frac{K - 1}{P} \log \frac{(Q - P)[Q + P - 2(K - 1)x]}{(Q + P)[Q - P - 2(K - 1)x]}$$

in which

$$K = \frac{k_1}{k_2},\ Q = [K(a + b) + (c + d)],\ P = \sqrt{Q^2 - 4(K - 1)(Kab - cd)}.$$

It is cumbrous to use and leaves ample opportunity for errors of calculation. In this case and in many others, notably in those of enzyme action, the experimental errors are unavoidably of considerable magnitude, far greater, probably, than the error of drawing a curve for the course of change and obtaining the rate of change $\frac{dx}{dt}$ directly by finding the values of the tangents to the curve at different points, say for 10, 15, 20, etc., per cent. change. This should be done not by *drawing* tangents as usually suggested but by means of a black silk thread. It is impossible to *draw* a straight line. By using a tight, black silk thread and reading off the values on the squared paper, we eliminate errors due to drawing and a crooked ruler; we can see both sides of

the thread which is an advantage and we can quickly get the mean of a number of independent readings. Such details make a vast amount of difference in practice. Besides the great advantage of simplicity of calculation afforded by the graphic method there are others of importance. One sees at once, for instance, any large experimental error or abnormality in any part of the interaction and one knows, further, just which part and how much of the interaction is being considered. This is most important, as it is dangerous to lose sight of the physical meaning of what one is doing, as can so easily happen when using complicated mathematical formulæ and equations. A chemist can hardly be expected to be responsible for his actions when he is out of his depth mathematically, as is so frequently the case.

The objection will naturally be raised that in the small book, for which there is the greatest demand, the mathematical discussion of chemical problems cannot be treated in the way here advocated. It cannot. But we have too many of the small science books which do all they can to breed mediocrity and foster superficiality. Books should be written on a proper scale or not at all. I do not wish to belittle in any way the value of books such as Mellor's *Higher Mathematics for Students of Chemistry and Physics* or his *Chemical Statics and Dynamics*—which have been of much service to chemists—or to throw cold water on the undoubted merits of Partington's new book on the mathematical side but rather to censure the general attitude of the physical-chemical school towards mathematics. In chemistry we have been served with too much mathematics flavoured with too little of the sauce of sound reason. There has been too much worship of mathematical formulæ and of the imposing appearance of mathematical proofs ; there has been too little consideration of the hypotheses and assumptions underlying so much of this mathematical splendour. Lord Kelvin said that " when you can measure what you are speaking about and can express it in numbers, you know something about it ; but when you cannot measure it, when you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind ; it may be the beginning of knowledge but you have scarcely in your thoughts advanced to the stage of science." It does not follow, however, because we express our ideas in numbers that our ideas are necessarily correct. So many of our numbers are not the numbers Lord

Kelvin speaks of at all—the numerical expression of experimental results—but numbers deduced from these on what may be entirely false assumptions. We must not forget that, as has so often been said, we cannot get out of the mathematical mill more than we put into it. If we put in doubtful assumptions we can get out only doubtful conclusions. When we look at the palatial mathematical edifices erected out of chemical facts by Arrhenius, Ostwald and Nernst on this side of the Atlantic and by their disciples Stieglitz, Acree, Noyes and Jones on the other side, we shudder to think of the terrific downfall should the foundations give way—such an occurrence is not impossible.

Consider our theories of solution. Almost all chemists regard electrolytes in solution as consisting of a number of units, some of which are active, others inactive—that the active ones have a fixed, unalterable activity while active and that change of activity of the solution, from whatever cause, is occasioned by a change in the mere *number* of active units. This is the assumption underlying almost the whole of the work that has been done on solutions of electrolytes and yet the assumption may be fundamentally wrong. We may find some day that all the units of the solute are potentially active and that change of condition changes not their number but their potential activity. Such a discovery would relegate to the rubbish-heap hundreds of tons of published matter.

WEEDS—THEIR PECULIARITIES AND DISTRIBUTION

By WINIFRED BRENCHLEY, D.Sc., F.L.S.
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The wild plants of the countryside, sometimes collectively designated "weeds," have always been of interest to the community at large. Years ago, before the science of medicine attained the position it now occupies, many remedies were obtained from wild herbs and shrubs; a few of these remedies linger on to the present day, especially in out-of-the-way districts. The old herbals and botanical works are full of accounts of the preparation and uses of the various essences and decoctions. Certain weeds still yield us some of our most valuable drugs, for instance, Belladonna, which is prepared from the poisonous Deadly Nightshade, whilst Henbane is obtained from the plant so named. The popular names of plants were frequently indicative of some special characteristic and habit, *e.g. Bindweed*, which climbs by twining round other plants; *Irongrass* or *Wireweed* (*Polygonum aviculare*), of which the stems are so tough that they offer great resistance to the passage of agricultural implements; *John-go-to-bed-at-noon* (*Tragopogon*), so named from its habit of closing at midday; *Poor Man's Weather-Glass*, which closes at the approach of rain.

Nowadays, at least to the initiated, the term "weed" is far more restricted in application, being applied only to those plants which interfere in a greater or less degree with agricultural and horticultural operations.

At all times and under all systems of agriculture the intrusion of weed plants into the crops has necessarily attracted attention. To ensure the greatest measure of success in agricultural practice, it is of the first importance that the crops should be kept clean; as the presence of alien plants or weeds militates against this end, a constant war has to be waged against the intruders. So the more information of all kinds and from all sources that can be gathered about such plants, the better the chance that the

farmer will have to cope successfully with their growth. The habit of growth as well as the habitat and climatic conditions all play their part in regulating the distribution of weeds.

At the outset some definition of the word "weed" is desirable, as the plants that are classified as such differ according as the land on which they grow is arable or pasture land. A good *pasture* bears a turf or herbage, consisting essentially of plants of nutritive value as animal food, which is either consumed on the field or cut and preserved as hay or silage. The larger proportion of such plants are grasses, though many others belonging to the Leguminosæ and other families find a definite place in the herbage. Under such circumstances, the term "weed" is necessarily somewhat elastic and adaptable, as some plants may be weeds in some places and under certain conditions whilst they may be welcomed and encouraged in other districts where different conditions prevail.

The weeds of pasture land are of various types:

1. Plants that are of a poisonous nature harmful to animals feeding on the herbage.

2. Species that are of very low nutritive value, which lower the quality of the herbage as food if they occur in any abundance.

3. Species that are valuable enough in themselves yet tend to occur in such superabundance that the balance of the composition of the herbage is destroyed, other plants of equal or greater value being crowded out more or less completely.

If this elastic definition be accepted, it is evident that pasture weeds may be of any nature. At times certain species of grass even will fall into the category, if for any reason they are deficient in value as food and occur too abundantly.

On *arable* land the conditions are different, as a rule; one species only is grown as the crop and the admixture of any other is undesirable and harmful. All such alien plants are classed as weeds, "plants growing in the wrong place"—perhaps they may be defined as "plants other than those sown, which grow up naturally with the crop and if unchecked prove exceedingly detrimental to the crop." The weeds are not always what are usually termed "wild" plants, *e.g.* if seed be shed at autumn time from a crop of barley and spring up among a succeeding crop of vetches, the intrusive barley must be designated as a weed.

On all land used for agricultural purposes, the plants—crops and weeds alike—are growing under somewhat artificial conditions and are considerably influenced by human agency. Tillage of the soil, the various manures used, the residual effects of previous crops, the cultivation of a variety of crops in rotation, are all factors that play their part in influencing the growth and development of the plants growing on any particular soil; the aim of the agriculturist is so to utilise these factors that the crop receives the greatest amount of aid and encouragement whilst the weeds are kept as much as possible in abeyance by unfavourable conditions of existence.

In cultivated ground weeds are obnoxious for several reasons:

1. They are plants of strong vital activity and if allowed to grow freely they rob the crop of a great deal of raw food material that would otherwise be left at its disposal. Pot experiments have been made[1] with sand, field soil and clay to test the relative demands made by certain plants upon the available food in the soil. The seeds of certain crops—rye, wheat, barley and oats—and of various common weeds—Charlock, Corn Cockle, Poppy, Sheep-sorrel, etc.—were germinated in different soils and the drain upon the soil measured by ascertaining the amount of carbon assimilated. It was found that assimilation was considerable during the first thirty days but that the weeds excelled the cereals in rate of growth, especially on soils containing a limited supply of plant food. The baneful effect of weeds is specially manifest on poor soils, as these suffer a greater proportional loss from the growth of weeds than better soils.

2. Weeds absorb the soil moisture and so lessen the supply of water available for the crop. Many weeds are luxuriantly leafy and give off a great amount of water from their leaves by transpiration, so that in a dry summer much water is wasted by the alien plants and the crop suffers from the effect of drought far more severely than would otherwise be the case.

3. If unchecked, weeds tend to grow very luxuriantly and to crowd the crop, robbing it of light and air, so that the plants do not have room or opportunity to attain their maximum development. Some of the most vital physiological processes—assimilation and respiration—are dependent upon a free access

[1] E. Vitek, "Weed Studies," *Monatsh. Landw.* **3** (1910), No. 11, pp. 333–44. Ref. in *Exp. Stat. Rec.* xxiv. p. 543.

of sunlight and air, so that anything that hinders this access is an agent by which the plant is practically more or less starved.

4. The weed seeds spoil the harvest of crop seeds by rendering them impure. The admixture necessitates special processes of separation and cleaning, entailing much expense. In addition, the seeds of some plants—Corn Cockle for example—are very poisonous, so the value of the crop for feeding purposes is very much lowered, as there is danger of poisoning stock if the animals are allowed access to such noxious plants.

5. Some weeds are particularly obnoxious as they choke up the threshing machine. The Wild Oat (*Avena fatua*) has a twisted and bent awn and the seed is also clothed with long hairs; these seeds get into the working parts of the threshing machine and cling together by their awns and hairs until they block every outlet, necessitating frequent stoppages to free the machinery.

6. Weeds provide harbourage for various insect and fungus pests which would either be entirely absent or at least far more easily dealt with if the ground were clear of alien plants. For instance, during the early summer months, when root-crops—Turnips, Swedes and Mangels—are practically lacking in the fields, the Turnip "fly," the Turnip-Gall Weevil and the Diamond-Back Moth all thrive on Cruciferous weeds such as Charlock, whereas they would die of starvation from lack of food were the weeds absent. Similarly, Thistles, Dandelions and Fat Hen afford cover for the Mangold Fly; whilst the Celery Fly perpetuates its existence by infesting the Cotton Thistle. The Cruciferous weeds also act as hosts for such fungus pests as "Finger and Toe" in Turnips, whilst the White Rot of Cabbages attacks the Shepherd's Purse, which indeed acts both as a host and as the initial source of infection for cultivated plants. Some of the Rust Fungi which work such havoc among cereal crops need two distinct hosts at different periods of their life histories and in some cases it happens that the second host is provided by certain species of weeds. Since the second host is essential the absence of the weeds would involve the dying out of the pest.

As stand has been made so constantly against the growth and spread of weeds, practically the only plants which reach any degree of significance are those provided with very efficient

means of reproduction. Reproduction may be either vegetative or from seed—sexual reproduction—and whilst most plants are chiefly dependent upon one of these two methods, a few of the worst weeds are equally well provided with both.

(*a*) *Sexual Reproduction.*—The weeds that occur frequently enough and are sufficiently distributed to become serious pests are usually notable for the large number of seeds produced by a single plant and many are provided also with an efficient distributing mechanism. The worst weeds in many places are the various kinds of *Charlock*, a species of Brassica, to which genus such cultivated plants as Cabbage, Turnip, Rape, Mustard and Swedes belong. The mature plant bears numerous seed pods, each containing several seeds[1]; on this account the species are among the most difficult to eradicate when once they are well established. The enormous quantities of seed produced are well shown by the regular armies of Charlock that spring up on any spot on which a rick has stood if in the previous year this weed has been garnered with the crop.

The Curled Dock (*Rumex crispus*) is another weed of which each individual produces many hundreds of seeds. Each seed or rather each fruit is enclosed in a little capsule made of three winged bracts, so that the seeds get scattered far and wide by the wind when they are detached from the parent plant. The same method of distribution of seed by means of winged fruits is seen in the Pennycress (*Thlaspi arvense*), though it is not so frequent among the low-growing herbaceous weed plants as it is among the tall trees like Elm, Maple and Lime. In the different species of Poppy each individual produces an enormous number of seeds unprovided with any mechanical devices but the distribution of the seeds is secured owing to the fact that they are exceedingly small and light; they are enclosed in capsules which open when ripe by means of pores or valves, so that in favourable weather the tiny seeds escape readily and are blown hither and thither by the wind.

The seeds of the various members of the order Compositæ, such as the Thistle, Dandelion, Sow Thistle and Hawkweed, are distributed by a very special method. The flower head in these plants is compound or "composite" in nature, being made up of numerous tiny florets crowded together and surrounded

[1] Nobbe states that as many as 4,000 seeds are produced on a single plant. See Long, *Journ. Bd. Agric.* xviii. No. 6, p. 461.

by a common involucre or envelope of bracts. The calyx of each floret is absent, its place being taken usually by a ring of fine hairs called a "pappus" which crowns the ovary and persists after the death of the floret. In some cases the pappus is inserted directly upon the ovary,[1] as in the Thistle, whilst in others it is raised up on a kind of stalk, as in the Dandelion, its function being the same in both cases; when the seeds are ripe, the hairs spread out and act like a tiny parachute, bearing the little fruits up on the wind, so that they are often carried over great distances. This peculiar mechanism also ensures that when the seed reaches the ground it shall enter the soil in the way that is most favourable to germination; its work done, the pappus becomes detached from the fruit and floats away. In other cases, as in the Willowherbs, which sometimes appear as field weeds, the hairs or plumes are different in origin and are attached to the seeds instead of to the fruit but they function in a similar way.

Other plants are provided with various types of hooks upon their fruits, so that animals such as sheep and cattle and also human beings aid in distributing them, as the hooks become entangled and the fruits carried from place to place; in this way the seeds are distributed far and wide. A most familiar example of this process is afforded by Cleavers (Klider or Goose grass) (*Galium aparine* and *G. tricorne*), the fruit of which is a regular little bur which sticks closely to anything that may touch it. The stems and leaves of this plant are also provided with recurved hooks, by means of which it scrambles and climbs over everything in its neighbourhood. The Enchanter's Nightshade of our gardens is another weed that possesses similar burs. The fruit of the Corn Buttercup (*Ranunculus arvensis*) is covered with large hooked spines, which make the plant a most troublesome weed, as the seed is so easily distributed.

(*b*) *Vegetative Reproduction.*—Whilst some of the weeds are common because of the large quantities of seed they produce and the special adaptations provided for its distribution, other plants are specially fitted to reproduce themselves vegetatively, increasing by means of certain extensions of the mother plants. Some do this by means of *runners* of different kinds, throwing out long thin stems bearing clusters of leaves at the joints or

[1] The ovary, when ripe, develops into the fruit, which encloses the seed or seeds In Compositæ each fruit is single-seeded.

nodes; roots then arise at each node, so that each rooted segment is capable of forming a new plant, which eventually may become detached from the parent plant and lead a separate existence. This is well seen in the Creeping Buttercup, the Wild Strawberry and the Silverweed. In other cases, the runners are hidden away beneath the surface of the soil, forming *rhizomes* or underground stems which push up aerial shoots at frequent intervals. One of the worst of such pests is the Couch or Twitch (*Triticum repens*), which sends out long underground stems bearing scale leaves from whose axils aerial branches spring up. The whole plant is most tenacious of life, every bit of the rhizome having the power when broken off of rooting itself and forming a new and independent plant. It seems as if nothing will kill it; the only means of eradicating it is to cultivate and remove most carefully every bit that is possible of the plant. The Enchanter's Nightshade, which is very common in woods and is also locally a serious pest in gardens, is chiefly reproduced in the same way, its long thin rhizomes penetrating the ground in every direction.

Another plant belonging to the Grass family is known as "Knotty Couch" or "Onion Couch"[1] because of the peculiar way in which it spreads. It is closely related to the Tall Oat-Grass and is in fact a sub-species of this genus. The base of the stem or rootstock swells up into thick knobs of which there may be several in a string one above the other. Each one of these knobs is capable of giving rise to a new plant if it be detached from its fellows. Consequently the Knotty Couch is as great a pest as is the true Couch. It has been supposed that the Onion Couch and the Tall Oat-Grass are really one and the same plant and that the knotted form is merely the result of local conditions, constitution of the soil and the amount of available soil moisture and rain; however, experiments have been made by growing seed from the two forms under various conditions of soil, moisture and light and it has been found that both types always come true to seed, so that it must be admitted that the Knotted form is a distinct variety which breeds true under different conditions and does not revert to the unknotted type.[2]

[1] *Arrhenatherum avenaceum var. tuberosum.*

[2] I am indebted to Miss Underwood, Rothamsted Experimental Station, for information relating to these experiments.

There is one particularly vicious weed which happily is not of very general occurrence—the so-called "Garlic" (*Allium vineale*). The writer's experience of this plant is confined to one district in the neighbourhood of Woburn (Beds) where it grows on very heavy clay land. It is a member of the Lily family and like all other plants in the order possesses a swollen underground shoot or bulb and is able to increase in the usual way by forming additional small bulbs at the base. Besides this, very numerous seeds are formed which are an efficient means of spreading the plant widely. But in addition to these two normal methods of reproduction, there is a third which is less usual. The flower-head consists of an umbel with a number of flowers springing from one point; at the base of the pedicels small bulbils are formed which ripen and become detached and on reaching the ground start growing, giving rise to new plants. Added to this, the plant possesses strong inherent vitality, so that it is most difficult to kill it and almost impossible to get rid of it without adopting very special measures.

It is noticeable that the plants which are the most widely spread and the most plentiful are those which are best provided with large quantities of seed and with special means of distribution. Such plants have another characteristic which is less obvious at first sight, in that they are very adaptable in nature and able to establish themselves and settle down happily under various conditions of soil and climate. On reading the account of some of the Canadian weeds[1] one is struck by the number of weeds that are reported as "introduced from Europe," over a score of such being described, including some of our most common pests—Curled Dock, Spurrey, Charlock, Creeping Thistle and Corn Cockle. All these plants have established themselves so well that they are now as troublesome in Canada as they are in England. Another striking fact is that many of the common British weeds are native in Canada and that these are as serious nuisances there as here. The Wild Oat, Couch, Sheep's Sorrel, Fat Hen (*Chenopodium album*), Bindweed, Dodder, Field Sow Thistle and Bladder Campion are among the species which are common in both countries.

[1] J. E. Hewitt, *Weeds of Ontario*, 1911; G. H. Clark and J. Fletcher, *Farm Weeds of Canada*, 1909.

The question of the distribution and association of weeds is one of great economic importance, though it is only during comparatively recent years that much definite and accurate work has been done in this field. At the present time, whilst a good deal of positive and exact information is available with reference to the weeds of pastures, our knowledge of the weeds of arable land is still fragmentary and more or less disconnected.

During the last fifty-five years experiments have been carried out at Rothamsted upon permanent *pasture* land to determine the relations existing between the plants and the soil on which they grow under the influence of various manures; the growth of different species in competition has been exhaustively tested in the course of these experiments. With regard to *arable* land, whilst much is known about the individuality of different weeds and many references exist as to the soils on which certain plants have been noticed, very little is known about the actual association between the different species on different types of soil in conjunction with various farm crops. Many observers have been at work but hitherto their observations have not been worked up systematically and so tabulated and reduced as to give the information required in a concise form; consequently our knowledge in this respect is still somewhat indefinite and vague.

An attempt is therefore now being made to collect definite and precise information as to soils, weeds and crops in different parts of the country. Work is being done during the summer months on soils derived from different geological formations to determine, as far as possible:

(*a*) Whether certain weeds are definitely associated with particular types of soil (clay, chalk, sand, etc.) or whether the nature of the soil is immaterial;

(*b*) Whether the association, if it exists, is constant or whether it varies according to the locality, treatment of the soil, etc.

(*c*) Whether the geological origin of the soil plays any part or whether the texture of the soil is the determining factor;

(*d*) Whether there is any connection between the crops grown and the weeds which accompany them.

To this end work has already been carried out in Bedfordshire,[1] in the district between Luton and Bedford, where the

[1] W. G. Brenchley, "The Weeds of Arable Land in Relation to the Soils in which they Grow" (I.), *Ann. Bot.* January 1911.

soils are derived in succession from the geological series of rocks comprising the Chalk, Gault, Lower Greensand and Oxford Clay; in Wiltshire, where the Chalk again reappears; and in Somerset to the south of Bath, on the heavy calcareous clay land overlying the Great Oolite, Fuller's Earth, Lower Oolite and the Lower Lias.[1]

In examining the results arrived at, it must be remembered that so far only a limited area comprising soils derived from very few geological series has been investigated and that until the work has been extended over wider areas, so as to embrace more numerous geological formations and also to include the same formation in different parts of the country, the conclusions must necessarily be recognised as being largely provisional and subject to modification.

Care has to be taken in this selection of districts to be examined. In some parts of the country, *e.g.* Norfolk and Suffolk,[2] the soil is not derived directly from the underlying rocks but is formed from drift that has been carried from other regions and deposited by glacial action; for the purposes of the immediate investigation such soils are disregarded, attention being confined to those that are as far as possible free from drift, so that the connection of the underlying rock with the weed flora can be more easily determined. The plan adopted is to visit as many as possible of the farms scattered over the area under investigation, during the summer months, from about the end of May, when the weeds are beginning to make progress, up to the beginning of harvest time. The fields are thoroughly traversed and searched, every species of weed occurring in each field being noted, as well as the nature of the soil and the crop grown; the relative prevalence of each weed is also indicated, the classification being into (1) dominant, (2) subdominant, (3) distributed, (4) occasional and (5) scarce or rare.

The distinction between the weeds growing actually with the crop and those in the adjoining hedgerows has been found usually to be very sharply marked; a few plants are common to both habitats but on the whole the hedgerow plants appear to have been unable to enlarge their borders by spreading into

[1] W. G. Brenchley, "The Weeds of Arable Land in Relation to the Soils in which they Grow" (II.), *Ann. Bot.* January 1912.

[2] F. W. Harmer, "Glacial Deposits of Norfolk and Suffolk," *Trans. Norfolk and Norwich Nat. Soc.* ix. 1910.

the fields. The weeds dealt with only include those found along with the crop. In each season's work (1910, 1911) over a hundred different species of weed plants, representing above seventy genera, have been met with. Some of these species occurred very frequently, being universally distributed in the localities in which they were found, whilst others, perhaps thirty in number, were only seen once or twice each year, being very local indeed in distribution. The two seasons were entirely different in character, that of 1910 being cold, wet and relatively deficient in sunshine, whilst that of 1911 was exceptionally hot and sunny, the deficiency in the rainfall causing conditions of drought to prevail during extended periods. It is quite possible that some of the results obtained are affected by seasonal variations and not only by soil conditions.

It is already evident that certain plants are quite universally distributed, occurring on any and every type of soil, whether chalk, clay or sand; others on the contrary are far more localised and entirely or chiefly confined to certain definite types of soil, so much so that particular species gradually come to be recognised as symptomatic of certain soils.

A. *Weeds of Clay Soils and Heavy Loams.*—The soils described as *clay* are very fine in texture, being composed essentially of a large proportion of tiny particles of silt bound together by a small proportion of true clay, silicate of alumina, which is most adhesive in nature. The finely grained texture renders the force of capillarity very great, as the interspaces between the particles are so small; as a result such soils are most retentive of water, holding a large percentage under ordinary conditions. This tends to make the clays somewhat heavy and cold; there is also a danger of water-logging, as superfluous water drains away with difficulty. Water-logged soils are peculiarly harmful to plant life, on account of the deficiency of air in their interspaces. Unless the under-drainage of clay soils is very efficient, so that the effect of these disadvantageous conditions is minimised, the number of species of plants found upon them is somewhat restricted, as all plants which are at all sensitive to an excess of soil moisture are lacking, leaving the field clear for those which are less exacting in their demands. The flora on the clays, however, is not very sharply marked out, as there are so many gradations from heavy clay down to heavy loams and thence to lighter loams.

A good *loam* provides the type of soil which is the most favourable to plant growth. The loams are intermediate in texture between sand and clay and the disadvantages of both the latter are eliminated by the admixture. A finely grained soil containing plenty of humus (derived from decayed vegetable matter) retains a sufficient supply of water to encourage the growth of the majority of plants, provided the subsoil be sufficiently permeable to admit of good drainage and that water-logging be avoided. Such a soil, whilst it contains all the moisture the plant needs, also has sufficient air in its interspaces to enable the roots to breathe freely, so that, other things being equal, all the vital processes can go on satisfactorily. Consequently the great majority of species are to be found on soils of this type, the relative dominance or comparative scarcity or absence being determined by the different local conditions.

The weed floras of the heavy soils are characterised at the outset by the fact that there are very few species which can be said to be symptomatic, *i.e.* there are practically no plants which are confined to clay, appearing nowhere else. The number of species occurring is less than on the lighter types of soil and they are such as are more or less universally distributed, though in different districts some plants show a greater or less predilection for heavy land. Still, though the species are restricted in number, the actual quantity of weed present is equally great. It frequently happens that a weed which is characteristic of a certain type of soil in one part of the country may in another be conspicuous by its absence either from fields of the particular soil or from the whole district. For instance, the Scentless Mayweed (*Matricaria inodora*) is almost constantly associated with the clay soils investigated in Bedfordshire, whilst on the equally heavy land around Bath no trace of it is to be found. The explanation in this case may be that the plant is shy of lime, as the eastern clay is non-calcareous in composition. Again, in Bedfordshire the Hoary Plantain (*Plantago media*) is practically absent as a cornfield weed, whereas in the Somerset district it occurs frequently and is the one species that is only found on clay. This again may be accounted for by the fact that the plant is notably a denizen of limestone districts,[1] as the Oolitic series of clays around Bath contain much limestone thus providing a congenial soil for this plantain.

[1] Bentham and Hooker, *Handbook of the British Flora*, p. 366.

The Red Bartsia (*Bartsia Odontites*) affords a curious instance of change of habitat in different places. In the eastern county it is a characteristic clay weed, being practically confined to such soil; it is also presumably a chalk-hater since it is never recorded as growing on the soils of the chalk downs. In Wiltshire, on the contrary, it is rare on clay but a frequent and characteristic plant of the calcareous soils, so entirely belying its character. This total inversion of habitat is apparently rare, as very few instances have so far been met with. Cases of partial inversion, in which plants of localised occurrence in one district are more generally distributed in another, are fairly common. A few of the weeds that are most generally associated with clay soils are Charlock (*Brassica Sinapis*), Greater Plantain (*Plantago major*), Knotgrass (*Polygonum aviculare*), Black Bindweed (*Polygonum Convolvulus*), Wild Mint (*Mentha arvensis*) and the Corn Buttercup (*Ranunculus arvensis*); all these weeds are to be found on other types of soil as well.

B. *Weeds of Sand and Light Loams.*—Sandy soils provide conditions exactly opposite to those appertaining to clay soils. They are composed of a considerable proportion of quite large particles with some admixture of silt; owing to the size of these particles, however, the silicate of alumina, which is generally present, is unable to bind the soil together as in the case of the clays. Instead of being adhesive, the sandy soils are very loose and friable; the interspaces between the particles are fairly large and so the force of capillarity is relatively small; the soil is also generally very poor in humus or decayed organic matter and as humus is exceedingly retentive of moisture, in its comparative absence the soil is unable to hold much water in its interspaces, so that if the subsoil is also fairly permeable the plants are subject to a considerable amount of drought at their roots. On true sands the species of plants that occur are practically only the Xerophytes, plants which are fitted with very special adaptations to enable them to withstand lack of water at the roots. On ordinary agricultural lands, true sands such as occur on the seashore are rarely in evidence, very sandy loams being the type usually met with. The species are somewhat characteristic and exhibit certain xerophilous characteristics in a greater or less degree in accordance with the degree of sandiness of the soil, which determines its water-holding capacity and consequently the condition of comparative drought.

The great feature of such sands and light loams, from an agricultural point of view, is that they lack lime; in fact they are frequently so deficient in this substance that they are sour or acid in nature—a state of things that is inimical to growing plants. Such acid soil conditions are reflected in the weed flora by the occurrence of a few very characteristic species which are unfailing indicators of acidity in the soil. The three most prominent of these are the Spurrey (*Spergula arvensis*), Sheep's Sorrel (*Rumex acetosella*) and Corn Marigold (*Chrysanthemum segetum*). Spurrey is perhaps the worst weed that a farmer has to contend with on such a soil. A member of the order Caryophyllaceæ, it produces an abundance of seed and spreads very rapidly, forming in many cases a regular undergrowth of vegetation. Long[1] states that if plentiful it may practically suppress spring- and summer-sown crops. Since it cannot grow unless the soil be acid, the most effective way of eradicating this weed is by applying chalk or lime, so reducing the acidity and thus rendering the soil obnoxious to the Spurrey, which dies out under the circumstances. Sheep's Sorrel is more frequently found in pastures than on arable land, though it does occur in the latter to some extent. The Corn Marigold with its bright yellow flowers and rather fleshy leaves is less common; the tendency among farmers is to associate it with light acid soils of a rather damp character.

Various other weeds are characteristic of light, sandy loams which are not necessarily acid, the chief of these being various species of Poppy, the Broad-leaved Dock (*Rumex obtusifolius*), the Red Dead-nettle (*Lamium purpureum*) and the Wild Chamomile (*Matricaria Chamomilla*). Besides these more characteristic species there are a number of weeds which are chiefly associated with these lighter soils but which also occur with more or less frequency upon other types.

A special instance of acid soil occurs in an unlikely situation. Away on the top of some of the Wiltshire downs, which are so markedly calcareous, is a thin layer of soil locally known as "bake," which is very sandy in texture and so totally deficient in lime that it is decidedly acid. This feature of the soil is indicated by the flora, which includes several species characteristic of sour land which are totally absent from the

[1] H. C. Long, "Identification and Eradication of some Common Weeds," *Journ. Bd. Agric.* xviii. No. 4, pp. 292–4.

chalk. Spurrey, Sheep's Sorrel and Annual Knawel (*Scleranthus annuus*) are all in evidence, whilst far more Scarlet Pimpernel is present than ever occurs on the calcareous soils. The transition from the chalk flora to the acid soil flora is remarkably abrupt and is accounted for by the thinness of the layer of "bake." The local theory is that this soil is derived from chalk but that all the lime has been removed by leaching, only an acid, sandy residue being left.

C. *Weeds of Chalk Soils.*—The calcareous soils are those derived from the Chalk rocks, both from the chalk proper and from the Chalk Marl underlying it. The soils vary from those which are essentially pure white chalk to those of the red lands and the chalk loams, which are rather heavier in texture but still truly calcareous.

The type of vegetation found upon chalky soils, especially on chalk downs, is very distinctive. Chalk, besides being of such a decidedly alkaline nature, provides a substratum of a peculiar type and naturally one would expect to see this reflected in its flora. There are certain species of plants to which chalk acts as a veritable poison, killing them outright. Such plants, in consequence, are absent from all such soils and receive the collective name of "calcifuges" or plants which flee from chalk. The Sweet Chestnut and some species of Pines (*Pinus Pinaster*) cannot tolerate chalky soils; consequently their presence in the vegetation is always an indication of the non-calcareous nature of the substratum. Plants of the same genus but of different species frequently behave quite differently in this respect. For instance, whilst *Pinus Pinaster* cannot tolerate chalk soil, *Pinus Laricio* prefers a lime-containing substratum and reaches a much better development in such a situation. It is not easy to determine the reason why lime acts as a poison to certain species of plants. *Lime-hating* plants grown on calcareous soils do not contain so much chlorophyll in their leaves as those grown on non-calcareous soils. The deficiency in the green colouring matter implies necessarily a decrease in the assimilating power of the plant and hence the process of nutrition is adversely affected, causing a condition of partial starvation. This may partly explain the toxic effect of chalk upon such plants.

The only species which are *really* absent from calcareous soils are those to which lime is absolutely poisonous. In the

case of other species the difference between a calcareous and a non-calcareous flora is not constant but varies with the locality. In other words, the mere fact that a certain species is absent from chalky soils in any one place is by no means a proof that the said species cannot tolerate lime, as in another district under different conditions but on similar soils it may be found quite commonly. So far as most plants are concerned the determining factor of their presence or absence on any particular soil is the struggle for existence. For instance, a species may be absolutely indifferent to the nature of the soil it grows upon, provided it be given a clear field, free from the competition of other plants. This "clear field," however, is rarely found in nature. Many plants are struggling for the possession of the soil and the general rule is that those plants that can make the best of existing conditions flourish the best and maintain their position, whilst any plants which cannot make such a brave fight for it or which suffer from any disadvantages of the situation tend to dwindle and die out in that particular place and under the particular conditions. Whilst any species, therefore, may be indifferent in itself to the nature of the substratum, if the soil tend to encourage other species greatly, the indifferent plant stands a good chance of being crowded out of existence; whereas if the soil be such as to discourage other species, the same indifferent plant gets its opportunity, flourishes well and may dominate the situation. Thus the effect of this struggle for existence explains why species that in one district are selective in their habitat appear in another on any kind of soil.

An examination of the weeds of the Chalk shows that there are a few plants which are very symptomatic of chalk when they occur in any quantity, the Bedfordshire result being largely borne out by the chalk flora of the Wiltshire downs. The Scabious (*Scabiosa arvensis*), Bladder Campion (*Silene Cucubalus*), Field Madder (*Sheradia arvensis*) and some species of Geranium and Brome-grass seem to be indicators of a calcareous soil. Another common and characteristic plant is Fumitory (*Fumaria officinalis*). This does not usually occur in any great abundance but occasionally it is absolutely rampant in some seasons and tends to smother everything else. In the Maiden's Bower, an old Roman encampment near Dunstable, this plant has sometimes been so prevalent that it was necessary

to hand-hoe the barley in order to save the crop. In 1910 the weed was very abundant but further search in 1911 revealed very few specimens of the plant, illustrating the great part that seasonal conditions play in affecting the prevalence of weeds. The Wild Pansy (*Viola tricolor*) is another noticeable species on chalk, though it is hardly symptomatic. This is a most variable plant, the flowers varying in size from tiny, insignificant things to fine specimens half an inch or more across, whilst the colour grades from white to deep blue. The suggestion has been made that the blue coloration indicates a substratum rich in nitrogen and that the paler the colour the less is the amount of available nitrogen present, so that the white form affords an indication that the soil is more or less deficient in this essential plant-food; this suggestion, however, awaits confirmation.

In Wiltshire the Shepherd's Needle (*Scandix pecten*) is a most noticeable feature on the chalk, especially on the heavier red land lying towards the top of the downs; the plant is a member of the order Umbelliferæ, its name being assigned to it on account of its big needle-like fruits which are ridiculously out of proportion to the minute, insignificant flowers which precede them. In some instances this weed is dominant, being present in very great abundance.

The Scarlet Pimpernel (*Anagallis arvensis*) and Fat Hen (*Chenopodium album*) provide further instances which show how differently one and the same species may behave on similar soils in different parts of the country. Both these plants are absent from the chalk soils in Bedfordshire, so much so that they were regarded as lime-haters. In Wiltshire Fat Hen is recorded more frequently from chalk than from any other habitat, whilst the Pimpernel is universally found on the chalk (in addition to other localities), although only very small quantities of it appear in such situations.

Whilst so many species of weeds can be associated with definite soils, there are a large number which are quite universally distributed, occurring on all types of soil; these may be dominant on any soil. This set of weeds include many of the very common species, those which are known to every one. Indeed, perhaps it is because of their universal distribution that they are so well known. Many of these, in addition to

being weeds of cultivated ground, are those most frequently found by the wayside and in hedges. Shepherd's Purse, Mouse-ear Chickweed and the ordinary Chickweed, the Woodbine or Wild Convolvulus, Cleavers, Groundsel, Sow Thistle, Creeping Thistle and the common Dandelion are all universally distributed in fields, no matter what the soil.

A consideration of the results set forth justifies the conclusion that the chief factor in determining the diversity of the weed flora is the texture of the soil rather than the difference in the amounts of plant food available. In other words, the degrees of coarseness or fineness of the soil and the correlated differences in water-holding capacity have far more effect in influencing the species of plants that occur than have the variations in the amounts of available phosphoric acid, potash, etc., present in the soil. The one chemical factor which has a profound influence is chalk or lime, which is far-reaching in its effects. This is most noticeable at the two extremes:

1. When lime is conspicuous by its absence, the soil being acid in consequence, a state of affairs that is definitely indicated by the flora;

2. When the soil is practically pure chalk, providing a very definite type of substratum and bearing a vegetation which again is quite characteristic.

Since lime is present in a greater or less proportion in nearly all soils, between these two extremes the texture of the soil is the predominant factor which determines the character of the flora.

So far we have been considering the weed flora solely in its relation to the nature of the soil but another factor to be considered is the character of the crop. Broadly speaking the field crops fall into three main divisions:

1. *Cereals*—Wheat, Barley, Oats.

2. *Roots*—Swedes, Turnips, Mangels and by courtesy Potatoes.

3. *Leguminous* crops or "seeds"—Clover, Sainfoin, Lucerne, Vetches, also Peas and Beans.

The habits of these three sets of plants are quite distinct and inevitably the difference influences the weed flora. The cereal crops at first give the weeds a very fair chance; later on, as they grow up and begin to fill out into ear, much of the

light and air is cut off and consequently the weeds suffer, though not before they have had time to reach a considerable development. The leguminous crops, on the other hand, tend to spread out laterally from the first and to cover the ground very closely, so that they choke out of existence many weeds that would otherwise be found. The difference in the method of cultivating the crops also affects weeds most vitally. From the very beginning, root crops are kept well cultivated, the weeds being constantly cut down by the hoe, so that few weeds representing few species occur among them, the presence of many weeds indicating poor cultivation. The dense covering growth of the Leguminosæ accounts for the absence of many species which are to be found associated with all other crops, such as Wild Mint, Plantain and Annual Meadow grass (*Poa annua*). Several species of plants, such as Fat Hen, Cleavers, Knot-grass, Chickweed, etc., were absent or very rare in seed crops in Bedfordshire whilst they were frequently met with in association with them in the West. This may probably be accounted for, as many of the seed crops of 1911 had been unable to make much headway after they were sown, owing to the prolonged drought, so that the weeds had a fair chance to get ahead before the crop was big enough to smother them out. A few plants, on the other hand, are almost constantly associated with seed crops and indeed are confined to them; such are the Thyme-leaved Sandwort (*Arenaria serpyllifolia*), Mouse-ear Chickweed, Wild Madder (*Sheradia arvensis*) and species of Geranium. It is a moot question whether these species are always indigenous to the localities in which they occur, as when they are found in seed crops and nowhere else in the neighbourhood it seems probable that in many cases they have been introduced from other places and sown with the crop seed.

Parasitic Weeds

There is one class of plants which is of peculiar interest from an agricultural point of view, on account of the curious mode in which they live and because when the plants occur in quantity they are so very detrimental to the well-being and life of the crop. These are the *parasites* or plants which live at the expense of others instead of leading independent lives. All normal plants obtain their raw food material first hand from mineral substances dissolved in the soil

water which is taken up by the roots and from the carbonic acid gas absorbed from the air by the leaves and green parts of the plants. Under favourable circumstances, after translocation from the roots to the shoots, the raw materials in solution undergo certain chemical changes and are elaborated into substances which are capable of being used to nourish the plants. To effect assimilation two factors are essential, the presence of a sufficiently strong light in conjunction with chlorophyll, the green colouring matter of plants. The *parasites* as a class are either destitute of chlorophyll or contain very little of it but by association with other species of plants, known as "hosts," are able to derive their nutriment by specially devised means of robbery. Three of the commoner weed parasites are (1) Dodder (*Cuscuta trifolii*), (2) Yellow Rattle (*Rhinanthus crista-galli*), (3) Broomrape (*Orobanche minor*).

1. *Dodder.*—This plant, which is closely related to the Bindweed, is of annual duration and as an agricultural weed is chiefly parasitic on clover and similar leguminous plants. The seed germinates in the soil and produces an undifferentiated threadlike seedling, which pushes up into the air and twists about until it finds a living plant or host to which it can attach itself; the Dodder twines round the leaves and stem of its host and finally, when attached, dies off itself at the base, so that all connection with the soil is lost. Numerous small suckers or "haustoria" are sent out which penetrate the tissues of the attacked plant and act as sucking organs by means of which elaborated food material is transferred from the host to the parasite. The Dodder is reddish or yellow in colour, never green, as no chlorophyll is formed and no leaves are produced; the long thin straggly stems develop an abundance of dense clusters of small whitish flowers, which are succeeded by rounded brown seed-pods. Large quantities of seeds are produced, so that the absence of Dodder seed from any crop seed which is intended for sowing is essential if a clean crop is to be obtained. If the Dodder secure a good hold it kills its host plant by strangling it and by robbing it of food, thus causing it to starve. It spreads rapidly, throwing its straggly stems from plant to plant; if unchecked, it may easily cover a whole field of clover and kill it entirely.

2. *Yellow Rattle.*—In contrast to the last this is only a semi-parasite in that it does bear green leaves but apparently the

chlorophyll in these is deficient, probably in quantity and also in quality, so that the plant is unable to assimilate sufficient carbon to supply itself with all the food it requires; consequently, a parasitic habit of life has been adopted, the plant living largely at the expense of certain hosts. It is usually found in pastures parasitic on the roots of grasses; the plant is generally of small stature and is sometimes classed as a distinct variety—*Rhinanthus minor*. A larger variety, *R. major*, occurs among cereal crops, where it may do great damage.[1] The plant possesses fibrous roots which develop fairly large spherical suckers and these attach themselves to the roots of the host plant. Absorption cells are developed on the suckers which rob the host of nutriment, in aggravated cases causing them to die of starvation. Even when the plant is not killed off, it is very much impoverished by the drain instituted by the parasite. The Rattle may do harm in another way. Large quantities of seeds are produced which fall in close proximity to the mother plant. Consequently thousands of seedlings spring up within a small area just at the time when the young plants of the crop stand most in need of light and air, so that the latter are stinted in these essentials and do not flourish so well as they otherwise would do.

3. *Lesser Broomrape.*—This plant again is a total parasite but of a type quite different from that of the Dodder. The plant consists of a sturdy stem of a dirty brown or purplish colour, with scales to match; no true leaves are produced and no chlorophyll. Each stem bears a spike of flowers resembling it in colour, each flower being furnished with a bract similar to the scale leaves; numerous small seeds are produced. Instead of possessing a true root, the base of the plant is swollen just where it is attached to the roots of the host plant, which belongs characteristically to the order Leguminoseæ, clover being a very general victim. The connection between the tissues of the host and parasite is so intimate that it is usually a matter of difficulty to distinguish between the cells of the two. Kerner and Oliver[2] state:

[1] A field of Barley at Heytesbury (Wilts) was so infested with Yellow Rattle this year (1911) that in many parts of the field the barley had practically disappeared, the plants being absolutely stunted. In other places where a certain amount of vegetative growth had been made, the plants failed to come into ear.

[2] A. Kerner and F. W. Oliver, *Natural History of Plants*, vol. i. pp. 184–5.

"The degree of union is such that one cannot even state with certainty where the epidermis of the nutrient root ceases and that of the Broomrape begins. The latter looks as if it were a branch growing out of the root it preys upon and this apparent fusion gave some colour to the view of the earlier botanists, who, ignorant of the life-history of these parasites, believed that they did not arise from seeds but were pathological outgrowths from the roots produced from their tainted juices; in other words, that they were 'pseudmorphs' sprouting from diseased roots in the place of leafy branches."

Happily the Broomrapes rarely occur in sufficient numbers to do much damage agriculturally; they are of interest chiefly on account of their peculiar appearance and mode of living. As no green leaves are produced, the plant is thrown back upon a parasitic life for its very existence. As so often happens with weed plants, the seeds retain their vitality during long periods. Experiments have been made[1] to test the vitality of the seeds of another species of Broomrape (*Orobanche crenata*) by carrying on germination tests in pots during a period of fourteen years; these have shown that the seeds lose practically all germinative ability after being eight years in the soil.

Weeds of Special Interest

Twitch or Couch.—Some of the most familiar words used in connection with agricultural weeds are "Twitch" and "Couch" but farmers will give curiously contradictory accounts of these pests. The reason for this apparent contradiction is that under these names are mustered quite a number of plants, all of which, however, are members of the Grass family. The true *Couch*, for which this particular name ought to be reserved, is *Triticum repens*; the long underground stems of this plant, bearing scale leaves and aerial branches, are so characteristic that they can hardly be mistaken. At least five other grasses are covered by the same name; in fact, it appears that any grass receives this general name which either runs along the surface of the soil just above or just below it or which spreads out much and covers any extent of ground with its branches. In Bedfordshire alone, besides *Triticum repens*, the White Bent-grass (*Agrostis stolonifera*), Field Foxtail or Black Bent (*Alopecurus agrestis*), Perennial Rye-grass (*Lolium perenne*), Catstail or Timothy (*Phleum*

[1] N. Passerini, *Abst. in Exp. Station Records, U.S.A.* xxiv. p. 30.

pratense) and Annual Meadow grass (*Poa annua*) all pass as Couch and Twitch. In the West Country apparently the name is reserved for the *Triticum*.

Coltsfoot and Field Horsetail.—The Coltsfoot (*Tussilago Farfara*) throws up flowering branches in the early spring as simple stalks, each bearing numerous small scale-like leaves and supporting a flower-head much like a small Dandelion; after the flowers have died down, very large leaves appear (out of all proportion to the size of the blossom) which are pale green in colour on the upper side and silvery white underneath. The *Horsetail* (*Equisetum arvense*) is easily recognised by its distinctive curiously jointed stems bearing at intervals whorls of jointed branches. The plant has no flowers in the ordinary sense of the word but produces instead a fructification consisting of a spike composed of several whorls of scale-like structures each of which bears cases filled with minute spores. The fruiting stems appear in the early spring and die down before the barren ones put in an appearance. The Coltsfoot and Horsetail are frequently found in association but investigation shows that there is really no connection between the two. Both species occur on all types of soil, whether they be heavy or light, calcareous or non-calcareous, though in Wiltshire the Coltsfoot is more prevalent on clay and they cannot be regarded as indicative or symptomatic. The view is sometimes held that the Horsetail grows where the soil is damp or badly drained but this hardly seems to be borne out by the observations hitherto made. Still whilst the character of the surface soil does not seem to be an influencing factor, it is quite possible that, wherever Coltsfoot and Equisetum do occur, underground water will always be found accessible to the roots of the plants, which are capable of penetrating to a considerable depth, being several feet long in some cases.

Mayweed.—Under the popular term "Mayweed" two distinct genera are included, *Matricaria* and *Anthemis*; of these again there are several species. The plants seem to be very impatient of competition. Wherever these weeds occur, if the crop is at all a heavy one, very little or no Mayweed is to be found in it, although large quantities will occur at the edges of the field, along lines of drainage or wherever there may be a clearing in the crop. Very probably these plants are peculiarly dependent

upon light and air, so that they are unable to flourish satisfactorily under the shadow of the crop even though all other conditions of the soil, moisture, etc., are suitably adapted to their well-being. The complete absence of *Matricaria* on the heavy lands of Somerset has already been mentioned.

Charlock.—The various weeds known as Charlock are very widespread and very dominant in many places. Quite a number of the Cabbage tribe come under this heading but two of the most prevalent are *Brassica Sinapis* ("Charlock" proper) and *Brassica alba* ("Mustard Charlock" or "Wild Mustard"). Of the two, *Brassica Sinapis* is the more widely spread and in a greater or less degree is found upon every soil. *Brassica alba* is more of a chalk-loving plant and seems to flourish well where the soil is very chalky, though it is often found on sand and also appears on the calcareous clays near Bath. In some places, the change in the nature of the soil in different parts of the same locality is indicated by the change in the relative dominance of the two species. In the Dunstable district, where Charlock abounds, the foot of the hills is composed of a heavy chalk marl which is populated by *Brassica Sinapis* as the dominant weed; going up the slope of the hill the soil changes to pure chalk and *Brassica alba* (Wild Mustard) gradually takes the place of the true Charlock. In some parts of the Wiltshire downs where a layer of "bake" occurs above the chalk *B. sinapis* reappears, so that sometimes the variety of *Brassica* met with affords an indication of the nature of the soil.

Another curious feature of Charlock is the way in which it grows up under certain circumstances. If a well or deep hole be dug, Charlock frequently makes its appearance in the upturned soil, even in districts where this weed is quite infrequent. This is not always easy to account for. It is probable that the seeds are already in the soil in such cases but buried too deeply to germinate and that as soon as they are brought to the surface they start into growth. It is well known that certain seeds are able to lie dormant for extraordinarily long periods, often retaining their vitality during many years.

Many attempts have been made to devise some efficient means of eradicating Charlock and nowadays spraying is the method most in vogue. Frequently solutions of copper sulphate have been used but with varying success, as so much

depends upon methods of application and upon the weather prevailing at the time of spraying. A heavy thunderstorm soon after the process may render all the labour ineffectual. In some countries spraying is carried on with solutions of iron sulphate. During the last four or five years, this method of destroying Charlock in cereal crops has been largely practised in Denmark with most excellent results. At the present time, nearly all Danish agriculturists are adopting the plan. In some parts of England the best results have been obtained by continuous hand-pulling during many years but this method is now far too expensive to carry out efficiently, owing to the increased cost of labour and to the difficulty on account of legislative changes in getting boy labour for the purpose.

The foregoing sketch touches but lightly upon a few of the varied aspects of the Weed problem. The subject is many-sided and the time will come when it will be possible and necessary to attempt to co-ordinate the heterogeneous mass of information into a homogeneous whole. Meanwhile the importance of increasing our knowledge of every branch of the subject can hardly be over-estimated, since the question of food supply is so intimately bound up with it. The system of intensive culture by which the utmost output is exacted from each acre of ground is spreading rapidly and farmers are no longer content to leave their land to produce more or less what it will. But while the conditions are being so much improved and the growth of crops facilitated by efficient systems of cultivation and manuring, *pari passu* the weeds are gaining, as they are prepared to benefit quite as much as any crop from improvements in the soil and so the more intense the system of cultivation the more essential is it to fight down weeds in an efficient and scientific manner, as weed and crop cannot exist side by side without detriment to the latter more often than not in quality as well as quantity. Consequently the need is obvious of obtaining as much precise information as possible with regard to the habits, life-histories and associations of weeds, so that we may learn to attack them either at the weakest point in their structure or at the most vulnerable periods of their existence, thus rendering the task of suppression or eradication far easier and more effectual.

CONDITIONS OF CHEMICAL CHANGE

THE THERMAL DECOMPOSITION OF OZONE

By D. L. CHAPMAN, M.A.

As the current views on the ultimate structure of matter have been derived almost entirely from the study of the properties of matter in a rarefied condition, it would seem to be probable that the elucidation of the mechanism of chemical change can be best accomplished by the exhaustive examination of the phenomena which accompany interaction in the gaseous state. The importance of studying the dynamics of chemical change under such conditions has been realised and emphasised in the past by a group of English chemists whose researches have brought to light some striking and interesting facts. These facts have served to demonstrate that in most chemical transformations—even in the most simple—the processes are more complex than we should be led to infer if only the initial substances and the final products of the change are considered. When we glance at the hypotheses advanced in explanation of the facts, however, we cannot help being struck by their great diversity. That such diametrically opposite views can be held at the same time by competent authorities is in itself indisputable proof that the mechanism of chemical change is but imperfectly understood. Even the dynamic law of mass cannot, as yet, be said to have been established for gaseous changes ; the quantitative examination of the effect of so-called catalysts (which in most interactions play an important, perhaps an essential, part) has been undertaken only in a few isolated cases. Whether the remarkable accelerative effect of moisture is proportional to its concentration is unknown. That progress should be retarded by lack of knowledge on such fundamental points is scarcely surprising ; it is manifest that the essential quantitative information can only be procured by a very complete investigation being made of the conditions which affect the rates of a few simple chemical

changes. For this reason, H. E. Clarke, H. E. Jones and the writer have undertaken a research on the reconversion of ozone into oxygen; the results of this inquiry are brought together in the present article.

Although the classical memoirs of Andrews, Soret and Brodie deal mainly with the question of the constitution of ozone, they contain accounts of some interesting observations on the mode of production and on the stability of the compound. Andrews showed that the presence in the ozonised oxygen even of traces of the oxides of nitrogen will cause the ozone to revert to oxygen with great rapidity. Brodie discovered that the highest percentage of ozone could be prepared from oxygen which was kept in contact with concentrated sulphuric acid during the production of the ozone. He believed that this fact was due to the circumstance that ozone is more easily formed from dry oxygen than from the gas in a moist condition; but we shall see later on that the true explanation of the observation is to be sought in the circumstance that concentrated sulphuric acid absorbs the nitrogen peroxide produced, together with the ozone, by the influence of the silent discharge on the oxygen and the small amount of nitrogen which the latter contains. The difficulties experienced by the earlier workers in obtaining ozone from moist oxygen are now known to have arisen from the ineffectiveness of the silent discharge between the moist glass surfaces of the apparatus used by them, not from diminished stability of the ozone. Another remarkable fact recorded by Brodie, which still awaits a satisfactory explanation, is the very high proportion of ozone—as much as 85 per cent.—contained in the oxygen formed when carbon dioxide is acted on by the silent discharge.

An interesting modern investigation carried out with the sole object of elucidating the chemical properties of pure ozone was that of Shenstone and Cundall published in 1887. Convinced of the importance of working with highly purified material, these investigators took extreme precautions to obtain oxygen possessing a very high degree of purity and dryness. They proved that such oxygen is easily converted into ozone and they showed incidentally that dry ozone is completely destroyed by contact with mercury, after several hours, without oxidation of the mercury being perceptible such as occurs, to a greater or less extent, when the metal and imperfectly dried ozone come into contact. Seven years later, in 1894, Baker confirmed Shenstone

and Cundall's result that ozone is as rapidly formed from oxygen dried with phosphoric oxide as from the gas which had been desiccated with sulphuric acid only. Baker's observations on the action of dried ozone on mercury do not, however, harmonise with Shenstone's statement. In view of the fact that purified, dried chlorine does not appear to differ from the moist gas in its behaviour towards the metal mercury, it is, in the opinion of the writer, highly desirable that further experiments should be made on the action on mercury of ozone dried to different extents.

Shenstone was not satisfied with his earlier results; accordingly, in 1897, he returned to the examination of the main problem of his former research. In preparing the oxygen and in desiccating it he took the most minute precautions. The care taken to eliminate every conceivable source of error made it necessary to use a very complex and fragile apparatus, which unfortunately broke after three experiments had been performed. These experiments were considered sufficient, in view of the care that had been taken in carrying them out, to justify the conclusion that all previous statements on the subject of the research were wrong. The result he obtained was that the stability of ozone prepared from oxygen thoroughly dried by exposure to phosphoric oxide is so small that only 0·2 per cent. of the oxygen can be changed into ozone by the silent discharge. On the other hand, ozone prepared from moist oxygen he found to be extremely stable at the ordinary temperature. Shenstone concluded that water vapour prevents the reversion of ozone into oxygen—that water, in fact, is in this case a negative catalyst. Armstrong refused to accept Shenstone's conclusion, his contention being that oxides of nitrogen had been formed by the action of the silent discharge on the oxygen and a small quantity of adventitious nitrogen contained in it and that the oxides of nitrogen, being powerful catalytic agents, rendered the ozone unstable. There can be no doubt that Armstrong's explanation is right and in the opinion of the writer Shenstone would have come to precisely the same conclusion if, instead of being satisfied with three experiments, he had investigated the effect of varying all the conditions of the experiment; he would then have discovered that the stability of the ozone was not only dependent on the amount of water vapour it contained but also on the length of time during which the oxygen had been submitted to the action of the discharge. It is not at first sight clear

why dry ozone becomes unstable much more rapidly than the moist gas under the influence of the discharge. This difficulty has, however, been quite recently removed through the discovery that in the presence of moisture the oxides of nitrogen and ozone are rapidly converted into nitric acid, which condenses on the surface of the glass and does not to an appreciable extent hasten the decomposition of ozone.

Before observations on any phenomenon can be used to draw from them conclusions in which any degree of confidence can be placed, it is essential that all the conditions capable of modifying the phenomenon should be known. The velocity of a chemical change can be increased by two causes: firstly, by contact action; secondly, by the catalytic agency of impurities, diluents and solvents. It ought, therefore, to be determined by experiment, in the first place, whether or not the interaction is one that takes place in the gaseous state; in the second place, what class of catalysts are capable of affecting it.

In the case of the decomposition of ozone in glass vessels, it is fortunately the case that the glass surface does not to any appreciable extent increase the rate of action, if the surface of the glass be not abnormally large in comparison with the volume content of the vessel. This fact has been established by H. E. Clarke and the writer by the simple expedient of heating ozonised oxygen in vessels of different size and thereby showing that the rate at which the concentration of the ozone diminishes is independent of the surface per unit volume of the enclosure in which the change occurs. This fact, that under the conditions observed the reversion of ozone to oxygen is a case of homogeneous change, *i.e.* of change affecting a single uniform state (the gaseous), of course simplifies the interpretation of the results and at the same time imparts to the conclusions a higher degree of trustworthiness; it enables us to investigate the much-debated problem of catalysis in gases under the most favourable conditions and to test the validity of the law of mass action.

The influence of different classes of impurities on the rate of destruction of ozone has been examined by H. E. Jones and the writer in some detail. The mode of procedure was to heat two samples of ozonised oxygen, one of which contained the impurity under investigation, at the same temperature in two similar glass vessels to which manometers were attached, the experiment being so conducted that the initial concentration of the ozone in

both vessels was the same. Some gases had no appreciable effect on the rate of decomposition of the ozone, whilst the rest, even when present in very small quantities, were found to accelerate the destruction of the gas enormously. This very marked difference in the behaviour of the various impurities is of interest because it points to the conclusion that the catalytic effects observed are the result of chemical action; for, if they originated in a purely physical influence, we should expect every substance to possess in some small degree the property of increasing the rate of the chemical transformation. Moreover, the gases chlorine and nitrogen peroxide, which have been shown to be powerful catalytic agents in determining the reconversion of ozone to oxygen, almost certainly form with ozone unstable addition compounds. The latter conclusion is based upon two facts. Firstly, iodine—an element closely allied to chlorine—is converted by ozone into iodic acid in the presence of water; secondly, nitrous acid is completely oxidised to nitric acid in the same circumstances. It is possible, therefore, that dry ozone forms with chlorine and nitrogen peroxide unstable ozonides which are very rapidly destroyed and give rise to oxygen. That the destructive effect of alkalies on ozone arises from a similar cause is undoubted; the following facts admit of no other interpretation. When ozone is passed into a concentrated solution of potash at a low temperature the colour of the solution changes to brownish yellow and no ozone escapes through the solution. The brown solution is extremely unstable and when it decomposes only potash and oxygen are formed. Moreover, if the brown solution be rapidly neutralised with acid, the temperature being kept low, not a trace of hydrogen peroxide can be detected in the solution. A concentrated solution of soda behaves in a precisely similar manner towards ozone. The brown solutions are therefore unstable ozonides. Further, the facts of organic chemistry speak eloquently in favour of the view that molecules can become more unstable or chemically active when united with other molecules. This view of catalysis in idiochemical changes is of course not novel, though direct evidence that catalysis in gaseous changes can arise from such a cause has hitherto been peculiarly defective. The deficiency of evidence in support of such a view has been the chief cause of Armstrong's postulation of the formation of addition compounds, in his theory concerning the influence of

moisture on chemical change, being regarded in many quarters as too speculative.

Other gases and particularly those which might be expected to be incapable of combining with ozone have been shown by Jones and the writer to be without influence on the decomposition of ozone. In the presence of these gases, the rate of decomposition of ozone is dependent only on the concentration of the ozone and the temperature. It will be convenient to consider separately the inert diluents, as our results are not in all cases entirely in agreement with those of previous workers.

Jahn maintains that his results demonstrate that the rate of decomposition of ozone is inversely proportional to the concentration of the oxygen mixed with the ozone and he has elaborated a theory of the mechanism of the decomposition of ozone based on this alleged fact. His hypothesis is that there exists between ozone molecules, oxygen molecules and oxygen atoms an equilibrium expressible by the equation :

$$O_3 \leftrightarrows O_2 + O ;$$

that this equilibrium is instantly established; and that the destruction of the ozone molecules is effected by interaction of ozone and the oxygen atoms according to the equation :

$$O_3 + O \rightarrow 2O_2,$$

the last change being a slow one and irreversible at a low temperature.

The hypothesis is a plausible one but the fact on which it is based is contested by Jones and the writer. The results (*J.C.S.* 1910, **97**, 2470) of a series of experiments, in all of which comparison with a control experiment was made, demonstrate that the concentration of the diluting oxygen is without effect on the rate at which the ozone is decomposed. Therefore, in our opinion, in accounting for the breaking up of the ozone molecules, the oxygen molecules can be left out of consideration.

Nitrogen and carbon dioxide have also been shown to be inert in promoting the reconversion of ozone into oxygen. That the latter gas should be inert is interesting, as it is proof that the high percentage of ozone found in the oxygen obtained by the decomposition of carbon dioxide by means of the silent discharge is not due to the ozone being rendered more stable by the presence of the carbon dioxide.

By far the most interesting conclusion drawn from our work

was that relating to the influence of water vapour on the change. It has been stated already that Shenstone found that ozone prepared from very carefully dried oxygen was extremely unstable and that Shenstone's result was accounted for by Armstrong by the assumption that oxides of nitrogen had been produced, in small quantity, in the dried gas by the action of the silent discharge on the oxygen and the small quantity of nitrogen with which it was contaminated. That Armstrong's explanation of Shenstone's result is correct has been recently established by a series of experiments by Jones and the writer. Accordingly the question as to whether or not moisture is essential for the destruction of ozone again becomes an open one. To settle the point, ozone which had been kept for some time in the presence of sulphuric acid (which is known to remove the oxides of nitrogen) was heated at 100° C. in the presence of varying amounts of water vapour. The rate of decomposition was found to be a constant for the same concentration of ozone and independent of the partial pressure of the water vapour. Moreover the constant of decomposition was the same when the gas had been thoroughly dried by long exposure to phosphorus pentoxide. So that we were forced to conclude not only that moisture is unessential for the decomposition of ozone but that it does not accelerate the rate of decomposition even to an appreciable extent. It would appear therefore that ozone can undergo decomposition without the assistance of a catalytic agent and that only those gases which can combine with ozone accelerate the rate of decomposition of this latter.

Experiments performed with the object of determining the order of the change showed that it was a little less than though almost bimolecular—a result which is in agreement with those of Jahn and Warburg.

We are therefore, I think, justified in assuming that the decomposition of ozone is in ordinary circumstances a chemical process of exceptional simplicity. It is the result of the formation of three molecules of oxygen from two molecules of ozone during a fruitful collision of the latter. It is, in fact, a change which can be correctly expressed by the simple equation:

$$2O_3 \rightarrow 3O_2.$$

If the above conclusions are justifiable, they have an important bearing on the much disputed problem of gaseous

catalysis and especially on the question of the remarkable effect exercised by water vapour on most chemical changes. It has been shown by Dixon, Baker and others that a very large number of chemical changes will not take place below a certain temperature unless water vapour be present and it has been concluded that water vapour is essential for the progress of these changes at all temperatures, indeed that the presence of water vapour is necessary in all cases of chemical change.

As it is probable that there is no really essential difference between the decomposition of ozone and any other simple chemical change, inasmuch as the decomposition of ozone can proceed without the assistance of water, the ground for the view that any change can proceed without the aid of water, provided that the temperature be sufficiently high, is considerably strengthened. In the opinion of the writer, the right view to take of the catalytic effect of water is that the vapour enables a change to take place at a temperature at which it could not proceed without its assistance: more precisely, that the rate of a chemical change is the sum of two terms, the first corresponding with the rate of change without the catalyst, the second with the increase in rate due to the catalyst; and that the first term can and often does become negligibly small in comparison with the second at low temperatures.

The mode in which the catalyst assists the chemical change is an entirely different question. Armstrong thinks that the powerful catalytic effect of moisture must be attributed to the fact that water is an effective electrolyte and that it can therefore introduce into the system the possibility of electrolysis taking place. The theory is in agreement with the principal facts. Chemical change is undoubtedly facilitated by the combined agency of an electrolyte and a metallic conductor, that is, by the process commonly known as electrolysis. If the metallic conductor be removed and the circuit be so small that an exchange of electrons can take place without its assistance (as Armstrong postulates), there is no ground for supposing that the electrolyte could not still aid the change to proceed. But on the other hand, if the metallic conductor be unnecessary, the only effect of its removal being that the change proceeds with greater difficulty, it seems to me that the electrolyte cannot be absolutely essential.

THE CORROSION OF IRON AND OTHER METALS: AN ADDENDUM

THE October number of the *Transactions of the Chemical Society* contains two communications by Messrs. W. R. Dunstan and J. R. Hill, the one entitled "The aerial oxidation (rusting) of metals," the other "The passivity of iron and certain other metals," which are of special interest as contributions to the subject of corrosion discussed in previous articles in this journal.

In his earlier publication, Dunstan came to the very definite conclusion "that the presence of carbonic acid or of any acid is not essential to the rusting of iron, for which only dissolved oxygen and iron are the necessary conditions." In describing his work, one of the points on which he laid particular stress was the manner in which not only alkalies but also a variety of salts, such as the potassium chromates, potassium iodide, permanganate and ferrocyanide, as well as hydrogen peroxide, prevent rusting. The communications under consideration are devoted to a discussion of the manner in which such inhibiting agents act. The conclusion now arrived at is that all agents which prevent rusting render iron *passive* and that the inhibition of rusting is the direct result of the passive condition of the iron.

The tests of passivity used were (*a*) a solution of nitric acid of the relative density 1·2, (*b*) a solution containing 0·5 per cent. of sulphate of copper; or the iron was immersed in distilled water and the time noted which elapsed until rusting began. Certain precautions, we learn, must be taken in applying these tests. After the iron has been exposed to the action of the agent, it must be washed with distilled water and not touched with the hands or shaken too violently, otherwise its passivity is destroyed. The best method is to remove the iron from the solution by means of a very thin glass rod bent into a hook at one end. The hook is inserted into a hole previously bored in the sheet of iron and the iron quickly dipped two or three times into distilled water; it is then transferred to the testing liquid and the rod removed.

A simpler method is to pour off the solution and rinse out the vessel (a glass jar is the most convenient) several times with distilled water; the testing liquid is then quickly poured over the iron so as to cover it. The whole process should be done as rapidly as possible and without unduly shaking the iron.

With regard to the discovery of passivity, whereas ordinary active iron is at once attacked by the diluted nitric acid, iron which has been rendered passive by treatment with a particular agent remains bright and unaffected by the acid during times varying from a few seconds to several hours; if it be touched with a piece of active iron or scratched with a sharp glass rod, action at once sets in, spreading from the point touched over the whole surface.

When active iron is introduced into a dilute solution of sulphate of copper, a film of copper is at once deposited; passive iron may remain hours in the solution unaffected and bright but if touched or scratched in the manner before described copper is at once deposited, the film spreading gradually from the point touched over the whole surface.

In distilled water exposed to the atmosphere, active iron usually begins to rust visibly within eight to ten minutes. Passive iron will sometimes remain bright during one to two hours before rusting begins and then the action is very local during a considerable time. The most uniform results are given with iron which has been etched with diluted nitric acid ($d = 1{\cdot}2$).

The results obtained by applying such tests to iron after exposure in various solutions are collected in the table on next page.

And not only iron but other metals—aluminium, copper, magnesium and zinc—are rendered passive, so that they resist aerial oxidation, by treatment with alkalies or with chromates and a number of other salts. The results are regarded by Dunstan and Hill as clear proof that in all cases the passivity is due to the formation of a film of oxide on the surface of each metal. Assuming this to be true, there is no novelty in the conclusion. It has long been recognised that the use of iron coated with zinc as a roofing material and that of aluminium is only possible by reason of the protection afforded by the coating of oxide on the surface of the metal.[1] The only rational explanation hitherto

[1] In the case of iron, the apparently special passivity caused by nitric acid may be due, if not to a particular mechanical or crystalline condition of the

Solution.	Strength per cent.	State of iron after exposure when tested in		
		Nitric acid, $d = 1{\cdot}2$.	Copper sulphate, 0·5 per cent.	Distilled water.
Chromic acid	0·01 to 1	Passive	Passive	Passive
Potassium dichromate	1	,,	,,	,,
,, chromate	1	,,	,,	,,
,, bromate	1	,,	,,	,,
,, iodate	1	,,	,,	,,
,, permanganate	0·5	,,	—	—
,, chlorate	4	,,	Passive	—
,, ferrocyanide	1	,,	,,	Passive
Sodium arsenate	1	,,	—	—
Hydrogen peroxide	1	,,	—	—
Potassium hydroxide	1	,,	Passive	Passive
Barium ,,	1	,,	,,	,,
Sodium carbonate	1	,,	,,	,,
,, ,,	0·13	,,	—	—
Potassium ,,	0·14	,,	—	—
Borax	1	,,	Passive	Passive
Ammonia	0·880	,,	,,	—
Sodium chloride	1	Active	Active	Active
,, sulphate	1	,,	,,	,,
Potassium chlorate	1	,,	,,	,,
,, nitrate	2	,,	,,	—
,, iodide	1	,,	,,	—

given of the passivity of iron is that the surface has been covered with a protecting layer of oxide; other explanations have been advanced but they all lack probability. In the case of aluminium, no other explanation seems possible.

The conclusions arrived at by Dunstan and Hill from the whole series of experiments under consideration are of special value perhaps as showing the attitude of mind of workers in this field and the need in which we stand of clear conceptions as to the nature of chemical change. They are verbally as follows:

"None of the existing theories adequately explains how the various inhibiting agents prevent rusting. A satisfactory

deposit, to the production of a resistant form of higher oxide. The insolubility in acids of ignited ferric oxide and of the magnetic oxide is well known.

The impenetrability of the layer of oxide on the surface of aluminium under certain conditions is very remarkable. Magnesium may be ignited in steam without difficulty and burns brilliantly when fired (Moody, *Chemical Society Proceedings*, 1891, p. 20). After giving the demonstration with magnesium, year after year, the writer has repeated the experiment with aluminium, in proof of the "thickness" of its skin: this year, for the first time, the metal caught fire and burnt brilliantly—as it should do. The reason of the different behaviour was not apparent.

explanation, however, is provided by the fact that all these agents, without exception, render iron passive and that this passivity persists after removal from the solution which caused it. Iron, so long as it is passive, does not rust in presence of oxygen and water. If exposed to atmospheric air, passive iron very gradually rusts, owing to the removal of the passivity of the iron by the carbon dioxide of the air. Passivity of metals may vary in amount and the amount of rusting subsequently induced will depend on the extent to which the passivity is removed.

"The mechanism of the process of the rusting of iron does not appear to differ essentially from the aerial oxidation of other metals. Similar reagents inhibit in all cases and traces of hydrogen peroxide can be detected when iron rusts, just as in the case of the other metals. All the metals concerned will, however, readily oxidise in solutions of potassium iodide and in certain other reducing agents which render the existence of hydrogen peroxide impossible. Consequently the hydrogen peroxide is probably not an intermediate product of the main reaction but may be the result of a secondary reaction, since the process of oxidation can apparently continue without its formation.

"We consider, therefore, that metals undergo aerial oxidation by direct action of oxygen dissolved in water. In the case of iron, ferrous hydroxide is first produced and is afterwards further oxidised to the ferric state, forming rust. Iron does not pass into solution before rust can be formed. The action of carbonic acid or of any other acid is separate and independent of the main process of rusting which proceeds in their absence.

"Several experiments which have been recently recorded as affording support to the old view that carbonic acid is necessary for the rusting of iron are explained by the results now recorded. The fact that iron which has been treated with dilute chromic acid or other inhibiting agents and subsequently washed does not rust in air free from carbon dioxide but begins to rust after carbon dioxide has been admitted is due to the destruction of the passivity of the iron through the action of the carbon dioxide.

"A similar explanation can be given of an experiment recorded by Friend (*Proceedings of the Chemical Society*, 1910, **26**, 179) and intended as a lecture experiment to demonstrate the necessity of carbonic acid for the rusting of iron. In this apparatus a hollow iron cylinder previously immersed in potassium hydroxide solution is shown afterwards not to rust in presence of water and air free from carbon dioxide. This result is due to the passivity of iron induced by the alkali and does not prove, as is assumed, that iron cannot rust unless carbon dioxide is present. If carbon dioxide is admitted, the

passivity of the iron is destroyed and rusting proceeds (see also Dunstan, *Nature*, 1911, p. 381)."

It would seem at first that a point has been scored by Dunstan and Hill in showing that iron and other metals are rendered "passive" even by treatment with alkali—but reflection shows that in giving this proof they have cut the ground from under their own feet and have disproved not only the correctness of their assumption that metals undergo aerial oxidation *by direct action of oxygen and water* but also of their assumption that acid is not necessary for rusting and that it neither conditions nor determines rusting.

The passivity induced by alkali is destroyed, we are told, by mere washing in four or five changes of distilled water—such passivity is scarcely worth speaking of: presumably it disappears because of the dissolution of the coating of oxide by the carbonic acid in the water. In any case such an observation is sufficient to dispose of the idea that the coating has particular resistant properties.

Moreover, assuming that in Friend's experiment the iron was superficially oxidised, if rusting be a process in which oxygen and water act directly, as Dunstan and Hill affirm, why did not the iron rust and continue to rust when kept in oxygen and water? Being so easily removed it cannot be supposed for one moment that the coating of oxide which conditions passivity produced by subjecting iron to the action of a weak alkali is of so special a character as to be rust-proof. The same argument applies to Moody's experiments. It must be held, in fact, that Moody and Friend have proved definitely that rusting is not a process of direct oxidation by oxygen dissolved in water. And as rusting takes place the moment acid is free to act together with oxygen and not before, it is difficult to see how it can be denied that acid is the determining cause of rusting and to be as necessary as oxygen. In point of fact, Dunstan and Hill seem to have supplied whatever evidence was needed to place it beyond doubt that rusting is an electrolytic process in which an acid is involved together with oxygen and water.

They seem to have difficulty in understanding the action of alkalies on iron as in discussing their experiments on passivity they remark:

"This (the removal of passivity on heating passive iron in hydrogen at 250°) is strong evidence in favour of the film being composed of an oxide of iron and this explanation is supported by the fact that many dilute acids which dissolve the oxides of iron also destroy passivity. It is more difficult, however, to explain the action of the alkalies on this basis. It is known, however, that under certain conditions iron can be oxidised by alkalies with the production of a visible film of oxide. Presumably such oxidation can only take place with the liberation of hydrogen and it has not so far been recognised that iron can react with cold dilute alkalies."

They also appear to have difficulty in understanding the process by which hydrogen peroxide is formed as they remark:

"In the case of iron a small quantity of hydrogen peroxide is formed during oxidation, just as with the other metals. The precise significance of the formation of hydrogen peroxide during the aerial oxidation of metals has not yet been discovered."

There is, however, no difficulty whatever in representing either process by the aid of the principles laid down in the previous articles. Traube many years ago explained the significance of the formation of hydrogen peroxide.

Zinc which carries the necessary electro-negative impurity is at once attacked by caustic soda, because zinc hydroxide has acidic properties and the formation of sodium zincate

$$Zn + 2NaOH = Zn(ONa)_2 + H_2$$

is an exothermic process—that is to say, one involving the liberation of energy, which takes the form either of electricity or of heat according to circumstances.

Ferrous hydroxide, $Fe(OH)_2$, has no acidic properties; presumably its formation would involve an endothermic change and therefore it cannot take place without assistance—unless energy be supplied from some external source. In presence of oxygen acting as depolariser, action takes place, as in the case of zinc, probably in the way represented by the equation

$$Fe + \begin{matrix}HONa\\HONa\end{matrix} + \begin{matrix}HOH\\HOH\end{matrix} + \begin{matrix}O\\\dot{O}\end{matrix} = Fe\begin{cases}OH\\OH\end{cases} + \begin{matrix}NaOH\\NaOH\end{matrix} + \begin{matrix}HO\\H\dot{O}\end{matrix}$$

It may be supposed that hydrogen peroxide is produced, as in all cases in which oxygen is active as depolariser and that in the case of iron it escapes notice probably because it at once undergoes decomposition at the instance of the ferrous hydroxide.

From this point of view, it is interesting that Dunstan and Hill appear to have been successful in detecting the presence of traces of the peroxide as a product of the rusting of iron.

Sodium chloride and similar salts destroy the passivity of iron, so that rusting takes place in their presence; sulphates and nitrates act similarly but less rapidly. It was pointed out in the previous article that such neutral salts should not promote rusting when alone. Dunstan and Hill have shown by experiment that this is the case and that the strong vivifying effect of sodium chloride is due to the combined action of both air and the salt. It is to be supposed, however, that the primary effect is due to acid—to carbonic acid derived from the air—as electrolysis would not set in, even though oxygen were present, if the metal were covered with a non-conducting layer of oxide.[1]

To summarise the position—there are three classes of compound, all electrolytes, with which iron may be brought into contact under the ordinary conditions of practice: (1) *acids* (carbonic and sulphuric in particular), (2) *alkalies* (usually lime) and (3) *salts* (common salt, magnesium chloride, etc.); but as oxygen is ever present, iron is never brought into contact with these alone; moreover, acid is always associated with the oxygen owing to the presence of carbon dioxide in the air.

Iron is one of those metals on which—given the necessary electro-negative conductor—acids generally can act *unassisted*; the interaction is always of the type

$$Fe + 2HX = FeX_2 + H_2$$

In other words, as the iron is dissolved, the hydrogen of the acid is displaced by it; in presence of oxygen this hydrogen is more or less completely oxidised, according as the conditions are more or less those which favour effective depolarisation.

Apparently, no such direct attack is possible when an alkali is used alone, the combination not being sufficiently

[1] The corroding effect of salt in conjunction with carbonic acid often is not taken sufficiently into account in dealing with soft waters containing relatively large amounts of sodium chloride and little if any calcium carbonate. A case in point is that of the Goldfields Water Supply to Kalgoorlie, West Australia (see *The West Australian*, Perth, March 16, 1910). The suggestion made to free the water from oxygen before passing it into the steel main savours of the ridiculous rather than of the sublime.

powerful, the iron cannot displace hydrogen from the alkali and form a corresponding salt. Action takes place readily, however, when the assistance of oxygen is secured: though the iron alone cannot displace the hydrogen, when this latter has its attention engaged by oxygen and is attracted away, the iron easily slips into its place.

But the coating of hydroxide (or oxide) which is formed is superficial and perhaps almost of molecular dimensions, as the action comes to an end on account of the insolubility of the product (ferrous hydroxide or oxide) in the alkali. In the case of such a metal as zinc, the action of alkali is continuous—if the solution be of sufficient strength—on account of the solubility of the product. Again, no direct immediate attack is possible in the case of salts such as sodium chloride, as iron has not the power to displace the metal; it can only so act on salts of metals lower in the scale of energy than itself—such as copper, for example. When oxygen is included with the salt in the circuit, the salt is easily electrolysed: it is to be supposed that in a case such as that of sodium chloride, ferrous chloride is formed and that the iron does not become "passive" as the salt is soluble. Such action can only take place when there is no insulating film on the surface of the metal.

It should not be necessary to quote such elementary propositions as these. They are based on fundamental principles which should be the common property of all students of the elements of chemistry; our constant neglect to apply them is clear evidence of the unscientific and illogical way in which chemistry is taught.

At the present day, there should be no difficulty in interpreting the process of corrosion. What we most need to know, to make our knowledge of steel practical, is the relative corrodibility of the corrodible constituents—if there be several; also the manner in which the various electronegative constituents (not forgetting insulating impurities) are distributed and influence corrosion.

The metallographers do little to help us: they commemorate their labours by christening "ites" innumerable and they talk learnedly of phases but in the end give us very little more than phrases.

H. E. A.

MOTILE MECHANISMS IN HIGHER PLANTS

By J. BRETLAND FARMER, D.Sc., F.R.S.

Many persons who are in the habit of observing natural phenomena must have been occasionally struck by the rapid change in the appearance of a field of daisies when the sunlight becomes obscured by heavy clouds. The previously fully-expanded flowers "close up" rapidly. This is owing to the incurving of the green bracts which enclose the cluster of small florets constituting an individual daisy. Again, when certain thistles or common dandelions are fruiting, a shower of rain causes the white heads of thistle-down or the "dandelion clocks" almost to disappear; this occurrence is likewise due to the bending upwards and inwards of the bracts and the consequent compression and concealment of the fluffy fruitlets; these remain imprisoned until the return of fair weather, when the bracts again bend outwards and so allow the fruitlets to sail away on the wind.

An inquiry into the mechanism by which these and other analogous movements are effected has led to a recognition of the fact that the motile organ may be constructed on radically different principles in different plants; moreover, that whilst the direct intervention of the living protoplasm is responsible for many of them, in other cases the mechanism is of a more purely physical and less complex nature.

The mechanisms concerned directly in producing movement all depend upon alterations in the distribution of water but they may be grouped conveniently in three classes as follows:—

1. Protoplasmic.—These are related to changes in the condition of the protoplasm or its contents in the living cell. Such movements, which often occur in response to more or less definite stimuli, depend in many instances on modified conditions of turgescence, at any rate in those cases in which the movement of a multicellular tissue is concerned; there are other examples however, which are not so readily susceptible of explanation.

2. Hygroscopic Change.—This class of movements are caused

by the unequal expansion which occurs in the different cell membranes or parts of a membrane when wetted. The inequality of swelling (and correspondingly of contraction during drying) is related to a difference in the structure of the respective regions; it can often be detected in other ways than by the test of water absorption.

3. Cohesion of Water.—Mechanisms coming under this head depend upon the resistance to rupture which is exhibited by a mass of water when enclosed in a chamber the walls of which it can wet. In this class are to be found some of the most remarkable examples of delicately adjusted arrangements whereby the production of movements suitable for particular purposes is ensured. In all cases the cells concerned possess walls thickened locally. The thickened part is more or less distorted by the pull of the thinner portions as these are drawn inwards when water is withdrawn from the interior of the cell by evaporation or other means. The efficiency of this type of mechanism depends on the condition that the walls are impermeable to air; otherwise the continuity of the contained water would be impossible when the pressure within the cell is negative.

Although the mechanisms in the two last-mentioned classes may and often do work after the protoplasm has disappeared from the cells directly concerned, the actual structure whereby they *are* effective has been laid down previously by the living substance. These instances rather gain than lose in interest by this circumstance; in this respect they are paralleled by the water-conducting elements of the wood in which the conducting tissues are very complex and the complexity is definitely related to the functions they have to discharge. Nevertheless, the functions themselves are not assumed until after the disappearance of the protoplasm, which was the architect and builder of these same cells and tissues.

Botanists have long been familiar with the movements which are due to the response on the part of living protoplasm to external stimuli but in view of the numerous investigations of which they have formed the subject, it is surprising how very little we really know about them. This is even more true of the rhythmic movements frequently met with in growing plants which are not referable to any stimulating agent that can be detected readily.

In order to appreciate the general mode of working of these living cells a glance at their essential structure will not be out of place. A typical plant cell consists of an outer membrane, commonly composed of cellulose, which is extensible and also elastic within varying limits. Within this is the protoplasm, in form like a hollow bag completely filled with a solution of various substances. The cell wall and the protoplasm are freely permeable by water but the protoplasm does not allow the substances dissolved in the water contained within it to pass outwards; in fact, the protoplasm is only a partially permeable membrane and the dissolved substances, which are contained in the hollow (vacuole) of the bag, are the cause of the "osmotic tension" which is so characteristic and important a feature of the plant cell. By its means the protoplasm is tightly pressed up against the cell wall, which in its turn is thus forcibly stretched until the tension of the wall is in equilibrium with the osmotic pressure excited by the contents of the protoplasmic vacuole. This pressure may amount to as much as ten atmospheres and even more.

When living cells are freely supplied with water they absorb it and assume a turgid condition; the tissues built up of such cells can exhibit a considerable degree of rigidity.

Inasmuch as turgid cells are always stretched, it is evident that any agent which tends to reduce turgescence will effect a shortening of the tissues affected; also that the amount of contraction will be related to the elasticity of the wall. Furthermore, if the cells on one surface of a sheet of tissue are alone affected, the tissue as a whole will curve and become concave on the surface from which the water has been withdrawn; a corresponding change of form would result if the cells of one surface were so altered that the osmotic pressure of their contents was increased. A good example showing the influence of a one-sided alteration of pressure is afforded by the Tulip flower. Most people who have used these flowers for indoor decoration are aware that if brought from a cold into a warm room the flowers open widely—often much too widely—and that they will close again if transferred to a cool place. These movements, which formerly were attributed erroneously to uneven growth, are due primarily to a difference between the cells of the upper and lower surface in their relation to water and consequently in their turgescence. If the tulip be watched while the expansion is

going on, it will be noted that the movement occurs chiefly where the principal curvature is to be seen, near the base of the petals. That there is a difference between the cells of the upper and lower surface in respect of the way in which the water of turgescence is held may be shown easily by cutting a thin longitudinal slice through the median line of a petal and placing it in water. The cells will then assume their maximum of turgescence and the section will become incurved in the sense required to close the flower. On transference to a 3 or 4 per cent. solution of common salt they will lose water but more will be withdrawn from those of the lower face than from the upper (inner) and the section will straighten out, *i.e.* motion will be in the sense of opening the flower. These observations may be repeated many times with the same section, always with the corresponding result. The *active* movement is one of closure and depends on a sheet of cells situated near the outer (lower) surface of the petal. This is shown by killing the tissue by immersion in alcohol; whatever the position previously assumed the section will straighten out if now it be transferred to water.

There is some reason for believing that a sudden reduction of internal hydrostatic pressure (maintained by the presence of osmotically active substances dissolved in the vacuole fluid) is the proximate cause of many other bending movements; and seeing that a common result of stimulation is to provoke an increased respiratory activity, it is possible perhaps to regard the reduction of the amount of soluble carbohydrates, for example, as the physiological means which condition the movement.

The result might also be produced by the collocation of dissolved molecules into higher aggregates and it may be that such union is really connected with the slow movements often to be observed in growing and elongating organs.

A third possibility lies in a sudden or rhythmical alteration of the protoplasmic membrane itself, whereby it becomes more permeable and permits of the outward passage of at any rate some of the dissolved substances which are otherwise retained within the cell. That such effusion may happen during the rapid movements of the leaves and leaflets of the Sensitive Plant (Mimosa) and of the filaments of some stamens appears to be not at all improbable, especially in the light of Lepeschkin's experiments, though further investigation on this point is greatly to be desired.

Again, there seems to be no escape from the conclusion that the character of the protoplasmic membranes of certain cells may undergo rhythmical change in such a way as to allow the water, together with dissolved salts, to be forced out from them in certain determinate directions under the considerable pressure exerted by the elastic cell wall. No satisfactory explanation, however, has been given of the mechanism by which this excretory process is effected, though the fact itself can hardly be denied. Thus watery solutions are regularly and forcibly injected from the cells of the roots into the *dead* wood elements and this at a pressure of several atmospheres in some instances. Such a circumstance is the more remarkable when it is remembered that the cells which are so acting are in direct communication with those external cells which are wrenching the water away from the film which coats the minute particles of which soil is composed.

To return, however, to the mechanism of movement, a further survey shows that a differential turgor in adjacent layers of tissue does not afford an explanation of all the movements which are associated with living protoplasm.

For example, the tubular and often unicellular threads of certain moulds, such as Phycomyces, exhibit fairly considerable movements by virtue of which they bend in the direction of incident rays of light. In this respect, they resemble the stems of most of the higher plants; like them, they are specially sensitive to the more refrangible rays of the spectrum. But inasmuch as each thread consists of a single tube, it is manifest that no explanation based on unequal turgor in coherent and adjacent cell rows can be invoked here; to attribute the directive movement simply to the specific irritability of the protoplasm is mere circumlocution—and in such an explanation the question of mechanism is altogether ignored. Moreover, no satisfactory proof has been given of unilateral modification of the longitudinal extensibility of the wall, which might meet the case.

There are many other slow movements, *e.g.* the various curvatures of roots in response to the stimulus given by gravitation; concerning the causes which produce these, we know almost nothing. Much has been discovered as to the conditions which may affect the movements and something even of the subsidiary machinery in certain instances. The disturbance due to the action of gravity of the starch grains in the cells at the

tips of roots which are placed in new positions relatively to the vertical is very possibly the stimulus which sets the chemical processes in the root awry, as Czapek has shown. But why these should act in such a way as to produce the particular kind of movement which follows on the stimulus—and the actual direction of movement will depend on circumstances which are bound up with the organism as a whole—we do not know.

Much of what has here been said about these protoplasmic mechanisms and of the difficulty of realising the actual conditions under which they work is of course familiar to botanists who are concerned with the behaviour of living plants. They are also only too well acquainted with the experimental difficulties that confront the investigator and are aware of the fact that the ancillary sciences are as yet unable to afford much material assistance. It may be, however, that an attempt such as this to indicate even in the barest outline a few of the more obvious problems of one branch of plant physiology may not be wholly devoid of value.

In contrast with the obscurity that enshrouds the operations which are causative of movement in the living cell, the mechanisms which are not so directly concerned with the contemporary action of living protoplasm have been materially elucidated within the last fifteen years or so, mainly by the efforts of Steinbrinck, Kammerling, Schwendener and others.

The movements of various parts of the fruit by virtue of which the seeds are shot out, often to a considerable distance, had received full descriptive treatment at the hands of many of the older observers and differential tensions were freely and somewhat loosely invoked to explain them. But it is only within the last few years that we have gained an insight into the real nature of the mechanism concerned and have learnt to understand how it works.

Taking the movements due to hygroscopic change first, namely those depending on the unequal expansion or contraction which occurs in the cell walls of the different parts of the motile organ when it is wetted or dried, we find on the one hand that entire tissue groups are involved, on the other that only single cell membranes are concerned. All hygroscopic organs, however, possess in common the faculty of absorbing water unequally on opposite surfaces and so of curving inwards or outwards as the case may be. Examples of such hygroscopic motile structures

are afforded by many fruits, as for example the gorse, in which the valves separate suddenly from each other and curl up, thus causing the seeds to be flicked out to a considerable distance. The violet again exhibits a similar device, though the details differ. The opening of the three little windows in a campanula fruit is effected by small strips of tissue situated about equidistantly around the periphery of the capsule wall. The strips can be identified even in the unripe fruit as occupying a position at the bottom of three corresponding depressions in the wall. When the fruit ripens, they become torn away from the rest of the wall and roll backwards into the fruit cavity, thus leaving the openings and windows through which the seeds escape. When damp conditions supervene, each flap uncurls and closes up its corresponding aperture. This movement of opening and closing can be repeated many times with the same fruit and is due to the circumstance that the cell walls near the inner surface absorb (and give off) more water than those of the outer surface.

Another interesting example is afforded by the curious cruciferous plant known as the Rose of Jericho (Anastatica). When its seed pods are ripe, the plant dries up and its branches curl hygroscopically inwards; but when wetted, they again unfold and the fruits open and scatter the seeds; the plant remains rooted in the ground all the time and is not blown over desert sands, as has often been fancifully stated. Investigation of the structure of the stem shows that, though the wet and dry positions respectively are the reverse of those often assumed in the case of other plants, *e.g.* the thistle bracts mentioned above, nevertheless the actual mechanism is identical in principle in each case. A transverse section of the stem shows that the wood on the upper side of a branch of the Rose of Jericho differs from that on the lower side in containing far fewer vessels and considerably more fibrous tissue. Further examination of the fibres themselves on the upper and lower side of the stem respectively reveals other and significant differences of structure. The walls of the fibres are very thick and they show pitted markings on their surface; but whilst the general direction of the slit-like pits of the upper fibres is transverse, that of the pits on the lower fibres is mainly longitudinal or oblique. Now the arrangement of the pits affords a clue to the direction of greatest swelling. The wall behaves as if it were built up of brick-like particles (micellæ) between which water

can enter. The slit-like pits are parallel to the long axes of the "bricks" and it is evident therefore that the greatest swelling will be in the direction at right angles to the greatest length of the pit. In the Rose of Jericho, Steinbrinck was able to verify the anticipated difference between the extensibility of the fibres on the two sides. He found that when dried those of the upper surface shortened by about 8 to 9 per cent. of their length as measured when wet, whilst in the case of those on the lower

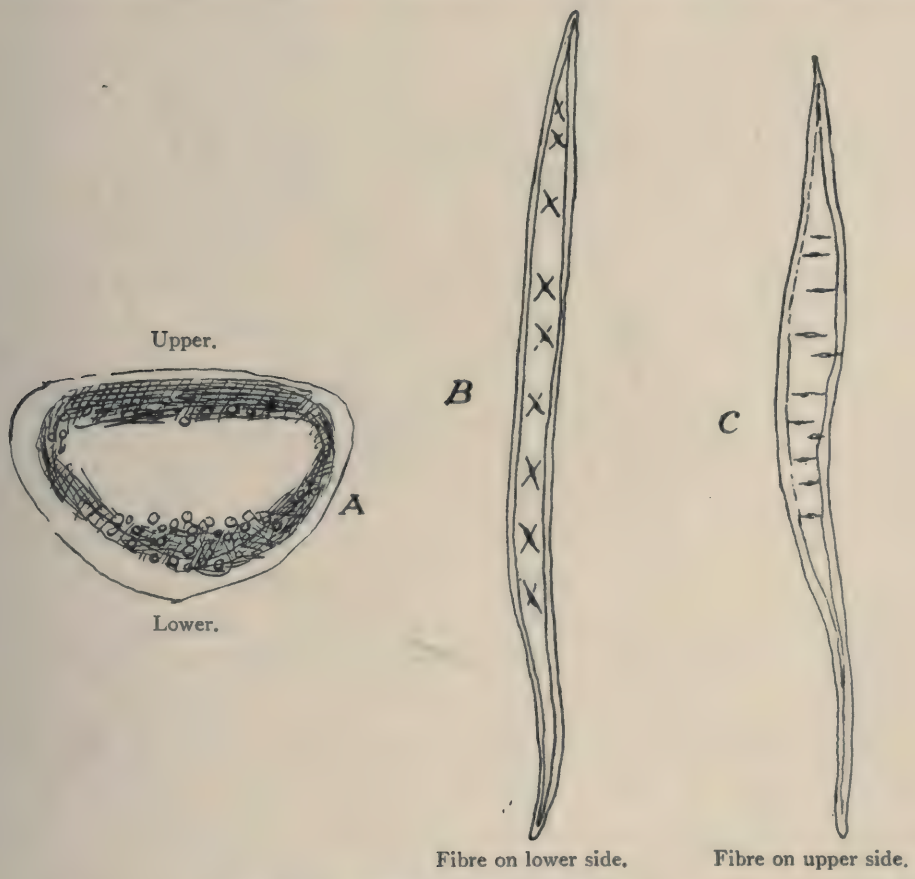

FIG. 1.—*Anastatica hierachuntica.*

Transverse section (A) and fibre from (B) under side, (C) upper side of the vascular ring.

side the stem hardly showed any change in length as between the wet and dry condition.

It thus at once becomes intelligible why the branches curl inwards on drying and straighten again when they are wetted.

The peristome of the moss capsule furnishes another beautiful example of a hygroscopic mechanism of a rather different character. The peristome consists of a ring of teeth, brightly coloured, fringing the orifice of the spore capsule or "fruit." There are commonly sixteen of these teeth (sometimes there are

two rows of them) and they are inserted around the rim of the bowl-like spore receptacle which forms the moss capsule. The teeth in many cases exhibit active and rather complex movements but the common moss, *Bryum argenteum*, provides a fairly simple example. If the ripe capsule of this moss be examined after the operculum (which covers the teeth like a candle extinguisher) has been removed, the mass of spores will be seen to be imprisoned by the infolded triangular teeth of the peristome curving inwardly towards a common centre above the spores. If the air be dry the teeth will presently curve outwards and downwards from the rim, leaving the spores fully exposed. The moisture of the breath is sufficient, however, to cause them instantly to bend upwards and centrally, so as again to cover

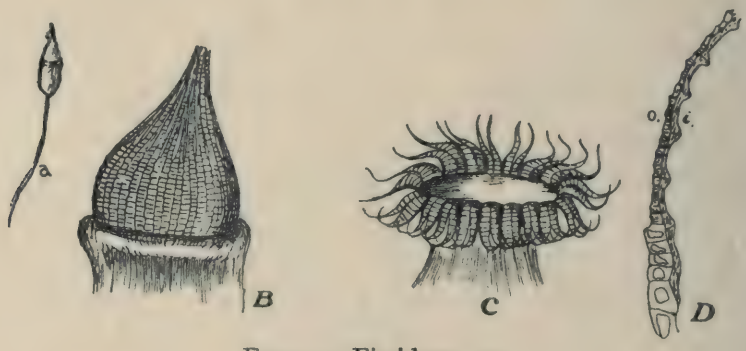

FIG. 2.—Fissidens, sp.

A, capsule; B, upper part of capsule with operculum removed, showing the peristome teeth in the damp position; C, peristome teeth in dry position; D, longitudinal section of peristome tooth, showing the different arrangements of the striae on the inner (*i*) and outer (*o*) surfaces.

the spores. The movements in this moss are very rapid and render it a favourable species for experiment. When one of the teeth is examined it is at once seen to consist of a membrane only and it is obvious that there are differences between the inner and outer surfaces; these are striated but whereas the striation runs longitudinally on the inner face, it is transverse on the outer surface. In some mosses these relations are reversed but the same corresponding difference is generally to be observed. As in the Rose of Jericho, the pits, so here the striation affords a criterion as to the direction of greatest absorption and loss of water and of the elongation or shortening of one surface of the peristome tooth; the circumstance that the striations differ in the manner indicated renders hygroscopic movements inevitable. The exact character of the movements in a particular case will vary according to the structure of the

membrane in its different regions; in some species of mosses they are very complex, the tips of the teeth bending over into the mass of spores and then, as the degree of moisture again varies, shovelling them out and dispersing them in the air.

The warping and twisting of timber is also due to structural conditions which involve the unequal capacity for swelling in a given direction possessed by the different elements that go to make up the complex tissue we call wood. There is room, however, for much more detailed investigation in this field and the information would be of practical importance in view of the technical utilisation of timber.

The forces developed in connection with some hygroscopic mechanisms are very great. The woody fruit of *Hura crepitans* may serve as an example. The fruit of this euphorbiaceous tree is a beautiful object, somewhat resembling a wheel. The seeds are borne in the outer thicker parts of the radially arranged segments. On the complete ripening of the fruit each segment splits during drying into two parts, which curve apart from each other, the whole process taking place with explosive violence. The fruit is thus shattered to pieces and the woody segments, as well as the liberated seeds, are flung to a considerable distance. So far as the present writer is aware, no measurements have been made of the disruptive force, due to the contraction consequent on the loss of imbibed water; to his knowledge it is sufficient to break a stout iron wire ring bound round the rim of the fruit.

3. Cohesion of Water.—A few years ago Dixon and Joly and almost simultaneously Askenasy showed that the property of cohesion possessed by water played a very important part in the physics of the ascent of sap and it soon became evident that mechanisms involving this principle were more widely spread in plants than was at first apprehended; furthermore, that phenomena which had been attributed to other agencies really belonged to this class. It is specially to Kammerling and to Steinbrinck that we owe the discovery that a considerable series of movements depend on cohesion and their studies have corrected the older views which regarded them as the result of hygroscopic differences in the membranes. In some instances it seems clear that both conditions really obtain but the effects of cohesion are so far more powerful that the hygroscopic qualities are practically negligible though they have a certain theoretical interest in another connection.

When a cell which is full of water is exposed to the air, evaporation will take place from the surface. The walls are saturated with water and this is of course continuous with the watery contents of the cell. If the wet walls are impervious to air, the thin cell walls will be brought closer to each other as water vapour passes off from the surface of the membrane. If the walls are rigid or thick, of course this approximation may not be possible; then very considerable stress is developed in the water and if any air happen to be dissolved in it, a bubble will be formed which will at once destroy the cohesion of the water. Other causes may also bring about the same result and the space will then not be filled with air but with water vapour, provided that no air can pass inwards through the wet walls.

It is clear then that with conditions of this kind actually or potentially existent in cells, movements may readily be produced in tissues, if the cell walls are rendered unequally rigid in a suitable way; investigation has proved that a large number of motile structures depend on a specific adaptedness of their constituent cells, which are thus enabled to utilise the cohesive quality of water in such a way as to produce a quite definite series of movements.

In all the examples at present investigated the movement is primarily secured by the pull inwards of a thin part of the cell wall exerted upon other thicker and therefore less extensible portions. The thickened parts of the wall exhibit a great diversity of pattern and their arrangement mainly determines the particular character of the movement in each case.

A fairly simple but very effective example of a mechanism of this kind is to be seen in the sporangium of an ordinary fern. The sporangium (of which several are borne together in clusters, on the under side of fern leaves), as is well known, is shaped like a biconvex lens and it is situated on a stalk; around the greater part of the rim the cells are more protuberant than elsewhere. The inner and party-walls are thickened in a remarkable way, whilst the outer walls are unthickened. This cell band forms the so-called annulus. As the water in the cells of the annulus gradually evaporates and—owing to the maturation of the sporangium—the loss ceases to be made good, a considerable tensile stress is developed within the cells and the unthickened elastic walls are pulled inwards; but this inwardly curving part of the membrane exerts a proportionally increasing pull on the

rigid party-walls that separate the cavities of the consecutive cells in the annulus. The general effect is to reverse the general curvature of this structure and to cause it to bend over backwards. Of course this can only be carried into effect by rupturing the lateral sporangial wall; the rupture also extends over the front edge just where a weak spot is always present. Then the upper part of the sporangium is rather slowly curled over and it consists of an opened bag containing the spores. As the tensile stress increases in the annular cells the continuity of the water is at last broken, whether by air passing in through the walls or in other ways. Instantly the annulus springs back to its original position, hurling out the spores from the sling-like bag as it does so. The surprising feature of this and similar

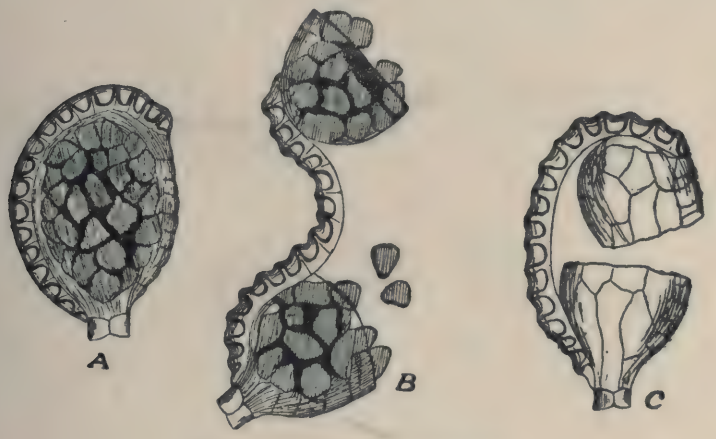

FIG. 3.—Fern Sporangium.

A, before dehiscence; B, in the act of dehiscing; C, after the ejection of the spores.

mechanisms is the perfection with which the details of structure are arranged so that the whole works like the mechanism designed by a skilful workman for the fulfilment of a definite and prearranged purpose.

Another remarkable example of a cohesion mechanism is furnished by the elaters which are found mixed up with the spores of most liverworts. The structures in question are spindle or eel-like cells pointed at each end and possessing one or more band-like coils of spiral thickening on the inner surface of their otherwise thin membranous walls.

The mode of working varies in the different forms and the degree in which it is due to water cohesion or to hygroscopic qualities also differs; we are chiefly concerned with the former.

If one opens a capsule of an Aneura and watches a clump of

spores and elaters under the microscope, the changes in the latter can be followed readily. The elaters should be damp at first and they may be taken either from fresh capsules or from one which has been preserved in weak spirit. The interior of the elater is filled with liquid; as drying takes place, the membranous parts of the wall sink inwards and the elater often wriggles as it becomes more and more deformed. Then, quite suddenly, it twists up and immediately straightens itself with a violent spring-like movement, which commonly jerks it out of the field of the microscope. If an elater which has gone through these changes be examined, it will be seen to be empty of liquid and apparently filled with air. But on wetting it again with water or weak spirit, the liquid is very rapidly absorbed and the "empty space" speedily contracts like a bubble of air under rapidly increasing pressure. Apparently, however, there is no

FIG. 4.—Elater of Liverwort (aneura) with a single spiral thickening band.

air present, only aqueous (or spirit) vapour, for the liquid supplied completely fills the elater in a very few seconds and no bubble is seen to escape. As the liquid dries again, precisely similar movements are again exhibited and they may be repeated many times. Sooner or later, however, air does leak through the membrane and then the elater manifests no further signs of motility or only the insignificant movements due to hygroscopic changes.

Kammerling, some years ago, showed that by cutting off the extreme end of an elater the power of movement was lost. This of course agrees perfectly with anticipation. Goebel, who as long ago as 1895 published an admirable account of the elaters in a number of genera of liverworts, remarked on the coincidence of the movement with the moment of the disappearance of their water contents and he further pointed out that in the different groups of liverworts the elaters exhibited considerable diversity in their behaviour. Some, *e.g.* in Fossombronia, are scarcely motile at all, whilst in others, such as Frullania, the slight independent movements the elaters are able to make

appear not to depend on a water-cohesion arrangement but on the hygroscopic character of the single band which is present in each of the elater cells. In this and a few other genera of liverworts the dispersion of the spores is chiefly accomplished by means of a kind of elastic mechanism. The elaters form cords which are attached by their ends to the wall and to the base of the sporangium, so that when the walls of the capsule split into the four diverging valves, the elaters are forcibly extended. They are then torn away from the walls and in springing back to their former length (or approximately so) they cause the spores to be jerked from the capsule.

Finally a glance may be cast at the mechanism by which the stamens of a flower shed their pollen. There are, as might be expected, considerable differences in detail but in the majority of flowers the general result is fairly similar. The pollen is commonly contained in four more or less tubular chambers situated in pairs on each side of the axis of the stamen. Dehiscence occurs down a line between the two chambers of each pan and the pollen is exposed by the curling back of the flap-like walls. The question arises as to the agency by which the dehiscence and curling back is effected.

When one watches an anther that is beginning to dehisce, it is easy to see that the formation of the slit is accompanied by the bulging out of a line of pollen grains ; the starting of the process was stated by Schneider to be due to the internal pressure of the pollen upon the walls, which when sufficiently large caused a passive rupture. This explanation does not, however, fit a very large number of examples nor does it take into account a very general peculiarity of structure in the anther. Just beneath the epidermis there is present a so-called " fibrous layer " of cells. In transverse section this appears to be formed of a large number of bars which extend from back to front on the radial walls of the cells of the sub-epidermal layer. On the back of the cell (*i.e.* the side abutting upon the anther cavity) the fibrous thickening is continuous, whilst on the outer (sub-epidermal) side of the cell it is either discontinuous or only feebly developed. It must be clearly understood that each of these " fibrous " cells contains nucleated protoplasm and this is bounded by a thin wall, the fibrous structure forming the locally thickened parts of this wall. In such a plant as the Christmas Rose (Hellebore), the thickening is more or less

continuous at the back wall and it stretches over the lateral ones somewhat like the fingers of a hand. Steinbrinck and others see in the fibrous layer the mechanism for opening and rolling back of the anther walls and they attribute its operation to the cohesion of the water contained in the individual cells of the fibrous tissue. As the anther ripens, more water is lost from this layer than can be made good from the adjacent tissues and the result is that the cell contents are in a state of stress and the cells themselves tend to become smaller. But inasmuch as the inner walls, as already explained, are practically rigid, owing to the extensive thickening present there, the reduction in size can only be effected by a collapsing inwards of the outer sub-epidermal wall and of the membranous parts of the lateral walls lying between the bars of thickening. These lateral walls, owing to the bars, are springy; they can converge as a whole towards the anterior end (though as soon as the internal stress is removed they might spring back to their former position). Such a convergence at the sub-epidermal end of the lateral walls would obviously bring about a stressed condition in the anther wall as a whole. The external surface would occupy a smaller area and equilibrium could only be restored by a curvature of the wall as a whole, whereby the originally outer surface would occupy the inner side of the curve, the outer being formed by the unaltered back walls of the fibrous layer. And this is just what actually happens.

It is thus highly probable that a correct explanation of the effective mechanism has been reached in many instances. The anther wall is held in a rolled-back position owing to the conditions imposed by the cohesion of water in the fibrous cells of the wall which have been compelled to assume a new and appropriate form by the force in question. If water is supplied to them they again become relaxed and the anther more or less closes again.

Nevertheless, there are difficulties in extending this "cohesion" explanation to meet every case. In the anthers of the Torch Plant (Kniphophia), for example, an arrangement of the fibrous tissue is encountered which is at first sight difficult to reconcile with it. Beneath the well-developed epidermis is a fibrous layer composed of rather irregularly shaped spindle-formed cells whose long axes run transversely round the cavity of the anther. The cells are provided with annular rings of thickening which

run completely round the thin walls. It would seem then that the effect of any diminution of the water content in this tissue would lead to a closer approximation of the rings as a whole, accompanied by a sinking in of the intervening unthickened parts of the membrane. And the effect of this over the whole tissue would be to produce not a rolling back but an inrolling of the flap. Such an inrolling of the edges just near the line of dehiscence may, as a matter of fact, be observed but still the flaps as a whole diverge and so open the pollen chambers. The explanation is to be sought in the fact that the thickened ring is more strongly developed at the back (towards the pollen cavity) than at the front (next the epidermis) of the cell. Furthermore, two adjacent rings are sometimes connected, others nearly so, by an extension of the thickening over the intervening part of the membrane at the back of the cell.

In such cases the spindle or maggot-like cells of which the fibrous tissue, in this plant, is composed would naturally tend to curve in such a way as to produce precisely the curvature of the anther wall as a whole which is actually observed. A further extension of the mechanism in this particular instance is seen in an increase, at the bases of the flaps, in the number of rows of annularly thickened cells.

That this is not a case of hygroscopic mechanism is proved by the fact that when the cohesion of the water is destroyed, *e.g.* by soaking in alcohol, the anther flaps curl inwards again and cannot afterwards be induced to open either by drying or by immersion in water.

One specially interesting example of an anther mechanism which acts like the fern sporangium may be adduced. The Castor Oil Plant was observed by Steinbrinck to jerk out its pollen forcibly. Opening of the pollen chamber is produced in much the same way as has already been described for other anthers but presently the elasticity of the walls overcomes the cohesion of the water in the cells. The result is a sudden break in the cohesion of the contained water; this is accompanied by an instantaneous reversal of the curvature of the anther wall and consequently the pollen is propelled from its surface.

No attempt has been made here to deal at all exhaustively with the recent work on the motile organs of plants but perhaps enough has been said to justify the statement in the earlier part of this article to the effect that the various mechanisms are con-

structed on several different principles. Those which depend upon the complex interaction of conditions in the living protoplasm have not received and probably are not likely soon to receive a full explanation ; but the more purely physical processes involved in the other two classes have lent themselves to a closer analysis, though, if closely regarded, the latter also will be seen to contain within themselves the jealously guarded secret of the how and why of their *adaptedness* to those functions which they are so well fitted to discharge. Indeed, from this point of view, perhaps they may even seem to be more difficult of comprehension than are those which result from the response of the living protoplasm to definite stimuli. For the structure on which the discharge of their functions depends has already been determined by conditions which were operative in the protoplasm at a period long anterior to that when the actual mechanisms become of specific use.

In fact the baffling problems of adaptation and adaptedness are just as real and just as exigent of solution in connection with the origin of the motile mechanism of a moss peristome or elater of a liverwort as they are in any of the other numerous manifestations of protoplasmic activity.

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ANIMAL SANCTUARIES IN LABRADOR[1]

By LIEUT.-COLONEL WILLIAM WOOD, F.R.S.C.

Though not a scientific expert on either animals, sanctuaries or Labrador, I can plead a life-long love of animals, a good deal of experience and study of them—especially down the Lower St. Lawrence—and considerable attention to sanctuaries in general and their suitability to Labrador in particular. Moreover, I can plead this most pressingly important fact, that a magnificent opportunity is fast slipping away before our very eyes there, without a single effort being made to seize it. I have repeatedly discussed the question with those best qualified to give sound advice and though I have been careful to consult men who regard such questions from very different points of view and books showing quite as wide a general divergence, I have found a remarkable consensus of opinion in favour of establishing a system of sanctuaries before it is too late.

Perhaps I might be allowed to explain that I speak simply as a Canadian. I am not connected with any of the material interests concerned. My only object is to prove, from verifiable facts, that animal life in Labrador is being recklessly and wantonly squandered; that this is detrimental to every one except the get-rich-quickly people who are ready to destroy any natural resources for ever in order to reap an immediate and selfish advantage; that sanctuaries will better conditions in every way and that the ultimate benefit to Canada—both in a material and a higher sense—will repay over and over again the small present expense required. And this repayment need not be long deferred. I can show that once the public grasps the issues at stake it will supply enough petitioners to move any government based on popular support and that the scheme itself will supply enough money to make the sanctuaries a national asset of the most paying kind and enough higher

[1] An address presented at the second annual meeting of the Commission of Conservation held at Quebec, January 1911.

human interest to make them priceless as a possession for ourselves and a heritage for all who come after.

Sanctuaries

A sanctuary may be defined as a place where man is passive and the rest of Nature active. Till quite recently Nature had her own sanctuaries where man either did not go at all or only as a tool-using animal in comparatively small numbers. But now, in this machinery age, there is no place left where man cannot go with overwhelming forces at his command. He can strangle to death all the nobler wild life in the world to-day. To-morrow he certainly will have done so, unless he exercise due foresight and self-control in the meantime. There is not the slightest doubt that birds and mammals are now being killed off much faster than they can breed. And it is always the largest and noblest forms of life that suffer most. The whales and elephants, lions and eagles go; the rats and flies and all mean parasites remain. This is inevitable in certain cases. But it is wanton killing off that I am speaking of. Civilised man begins by destroying the very forms of wild life he learns to appreciate most when he becomes still more civilised. The obvious remedy is to begin conservation at an earlier stage, when it is easier and better in every way, by enforcing laws for close seasons, game preserves, the selective protection of certain species and sanctuaries. I have just defined a sanctuary as a place where man is passive and the rest of Nature active. But this general definition is too absolute for any special case. The mere fact that man has to protect a sanctuary does away with his purely passive attitude. Then, he can be beneficially active by destroying pests and parasites like bot-flies or mosquitoes and by finding antidotes for diseases like the epidemic which periodically kills off the rabbits and thus starves many of the carnivora to death. But, except in cases where experiment has proved his intervention to be beneficial, the less he upsets the balance of Nature the better, even when he tries to be an earthly Providence.

In itself a sanctuary is a kind of wild "zoo" on a gigantic scale and under ideal conditions. As such it appeals to everyone interested in animals, from the greatest zoologist to the mere holiday tourist. Before concluding I shall give facts to show

how well worth while it would be to establish sanctuaries even if there were no other people to enjoy the benefits. Yet the strongest of all arguments is that sanctuaries, far from conflicting with other interests, actually further them. But unless we make these sanctuaries soon we shall be infamous for ever, as the one generation which defrauded posterity of all the preservable wild life that Nature took millions of years to evolve into its present beautiful perfection. Only a certain amount of animal life can exist in a certain area. The surplus must go outside. So sanctuaries are more than wild "zoos": they are overflowing reservoirs, fed by their own springs and feeding streams of life at every outlet. They serve not only those interested in animal life but those legitimately interested in animal death, for business, sport or food. I might mention many instances of successful sanctuaries, permanent or temporary, absolute or modified—the Algonquin, Rocky Mountains, Yoho, Glacier, Jasper and Laurentides in Canada; the Yellowstone, Yosemite, Grand Cañon, Olympus and Superior in the United States; also the sea-lions of California, the wonderful revival of ibex in Spain and deer in Maine and New Brunswick, the great preserves in Uganda, India and Ceylon, the selective work of Baron von Berlepsch in Germany, the curious result of taboo protection up the Nelson river and the effects on seafowl in cases as far apart in time and space as the guano islands under the Incas of Peru, Gardiner Island in the United States or the Bass Rock off the coast of Scotland.

Yet I do not ignore the difficulties. First, there is the universal difficulty of introducing or enforcing laws where there have been no operative laws before. Next, there is the difficulty of arousing public opinion on any subject, however worthy, which requires both insight and foresight. Then we must remember that protected species increasing beyond their special means of subsistence have to seek other kinds of food, sometimes with unfortunate results. And then there are the several special difficulties connected with Labrador. There are three British governments concerned—Newfoundland, the Dominion and the province of Quebec. There are French and American fishermen along the shore. The proper protection of some migratory species will require co-operation with the United States, perhaps with Mexico and South America for certain birds and even with Denmark for the Greenland seal.

Then there are the Indians, the whole trade in animal products, the necessity of not interfering with any legitimate development and the question of immediate expense, however small, for a deferred benefit, however great and near at hand. Finally, we must remember that scientific knowledge is not by any means adequate to deal with all the factors of the problem at once.

Labrador

But in spite of all these and many other difficulties, I firmly believe that Labrador is by far the best country in the world for the best kinds of sanctuary. The first time you are on a lee shore there, in a full gale, you may well be excused for shrinking back from the wild white line of devouring breakers. But when you actually make for them, you find the coast opening into archipelagoes of islands to let you safely through into the snug little "tickles," between island and mainland, where you can ride out the storm as well as you could in a landlocked harbour. This is typical of many another pleasant surprise. Labrador decidedly improves on acquaintance. The fogs have been grossly exaggerated. The Atlantic seaboard is clearer than the British Isles, which, by the way, lie in exactly the same latitudes. And the Gulf is far clearer than New Brunswick, Nova Scotia and the Banks. The climate is exceptionally healthy, the air a most invigorating tonic and the cold no greater than in many a civilised northern land. Besides, there is a considerable range of temperatures in a country whose extreme north and south lie 1,000 miles apart, one in the latitude of Greenland, the other in that of Paris. Taking the Labrador peninsula geographically, as including the whole area east of a line run up the Saguenay and on from Lake St. John to James Bay, it comprises 560,000 square miles—eleven Englands! The actual residents hardly number 20,000. About twice as many outsiders appear off the coasts at certain seasons. So it would take a tenfold increase, afloat and ashore, to make one human being to each square mile of land. All the same, wild life needs conservation there and needs it badly, as we shall presently see.

Most of Labrador is a rocky tableland, still rising from the depths, with some old beaches as much as 1,500 ft. above the present level of the sea. The St. Lawrence seaboard is famous for its rivers and forests. The Atlantic seaboard has the same

myriads of islands, is magnificently bold, is pierced by fiords unexcelled in Norway and crowned by mountains higher than any others east of the Rockies. Hamilton inlet runs in 150 miles. At Ramah the cliffs rise sheer 3,500 ft. and more. The Four Peaks, still untrodden by the foot of man, rise more than twice as high again. And the coloration, of every splendid hue, adds beauty to the grandeur of the scene. Inland there are lakes up to 100 miles long, big rivers by the score, deep canyons and foaming rapids—to say nothing of the countless waterfalls, of which the greatest equals two Niagaras. This vast country is accessible by sea on three sides and will soon be accessible by land on the fourth. It lies directly half-way between Great Britain and our own North-West and is 1,000 miles nearer London than New York is. Its timber, mines and water-power will be increasingly exploited. It should also become increasingly attractive to the best type of tourist, naturalist and sportsman. But supposing all this does happen. The mines, water-powers and lumbering will only create small towns and villages. There will surely be some conservation to have the forests used and not abused, especially by fire: and the white man should remember that he is the worst of all in turning a land from green to black. Except in the south-west and a few isolated spots, the country cannot be farmed. At the same time, the urban population must have communications with the outside world by which regular supplies can come in. This will make the settlers independent of wild life for necessary food; and wild life, in any case, would be too precarious if exploited in the usual way. The traders in wild-animal products, as well as the naturalists, sportsmen and tourists, are interested in keeping the rest of the country well stocked. So that, one way and another, the human and wild-animal life will not conflict, as they do where farming creates a widespread rural population or wanton destruction of forests ruins land and water and human and animal life have to suffer for it afterwards. All the different places required for business spheres of influence in the near future, added to all the business spheres of the present, can hardly exceed the area of one whole England, especially if all suitable areas are not thrown open simultaneously to lumbering, at the risk of the usual bad results. So there will remain ten other Englands, admirably fitted, in all respects, to grow wild

life in the most beneficial abundance and quite able to do so indefinitely, if a reasonable amount of general protection is combined with well-situated sanctuaries.

The fauna is much more richly varied than people who think of Labrador as nothing but an arctic barren are inclined to suppose. The fisheries have been known for centuries, especially the cod, which has a prerogative right to the simple word "fish." There are herring and lobsters in the Gulf, plenty of salmon and trout in most of the rivers, winninish in all the tributary waters of the Hamilton as well as in Lake St. John, whitefish in the lakes and so forth. Then, the stone-carrying chub is one of the most interesting creatures in the world. . . . But the fish and fisheries have problems of their own too great for incidental treatment; and I shall pass on to the birds and mammals.

Yet I must not forget the "flies"—who that has felt them once can ever forget them? Labrador is not a very happy hunting-ground for the entomologist. But all it lacks in variety of kinds it more than makes up in number of individuals, especially in the detestable trio of bot-flies, blackflies and mosquitoes. The bot-fly infests the caribou and will probably infest the reindeer. The blackfly and mosquito attack both man and beast in maddening millions. The mosquito is not malarious. But that is the only bad thing he is not. Destruction is "conservation" so far as "flies," parasites and disease germs are concerned.

Labrador has over 200 species of birds, from humming-birds and sanderlings to eagles, gannets, loons and herons. Among those able to hold their own, with proper encouragement, are the following: two loons, two murres, the puffin, guillemot, razor-billed auk, dovekie and pomarine jaeger; six gulls—ivory, kittiwake, glaucous, great black-back, herring and Bonaparte; two terns—arctic and common; the fulmar, two shearwaters, two cormorants, the red-breasted merganser and the gannet; seven ducks—the black, golden-eye, old squaw and harlequin, with the American, king and Greenland eiders; three scoters; four geese—snow, blue, brant and Canada; two phalaropes, several sandpipers, with the Hudsonian godwit and both yellowlegs; two snipes; five plovers; and the Eskimo and Hudsonian curlews. These two curlews should be absolutely closed to all shooting everywhere for several seasons. They

are on the verge of extinction; and it may even now be too late to save them. The great blue heron and American bittern are not common but less rare than they are supposed to be. Except for the willow and rock ptarmigans the land game-birds are not many in kind or numbers. There are a fair number of ruffled grouse in the south and more spruce grouse in the north. The birds of prey are well represented by a few golden and more bald-headed eagles, the American rough-legged and other hawks, the black and the white gyrfalcons, the osprey and eight owls, including the great horned owl, the boldest bird of all. The raven is widely distributed all the year round. Several woodpeckers, kingfishers, jays, bluebird, kingbird, chickadee, snow bunting; several sparrows, including, fortunately, the white-crowned, white-throat and song but now, unfortunately, the English as well. There are blackbirds, redpolls, a dozen warblers, the American robin, hermit thrush and ruby-throated humming-bird.

Both the land and sea mammals are of great importance. Several whales are well known. The Right is almost exterminated; but the Greenland or Bowhead is found along the edge of the ice in all Hudsonian waters. The Pollock is rare and the Sperm or Cachalot as nearly exterminated as the Right. But the Little-piked or *rostrata* is found inshore along the north and east, the Bottle-nose on the north, the Humpback on the east and south; and the Finback and Sulphur-bottom are common and widely distributed, especially on the east. The Little White whale or "White porpoise" is fairly common all round; the Killer is widely distributed but most numerous on the east, where the Narwhal is also found. The Harbour and Striped porpoises, and the Common and Bottle-nosed dolphins are chiefly on the east and south. There are six Seals—the Harbour, Ringed, Harp, Bearded, Grey and Hooded. The Harbour seal is also called the "Common" and the "Wise" seal and is the *vitulina* of zoology. It is common all round the coasts and the Indians of the interior assert that many live permanently in the lakes. Big and Little Seal Lakes are more than 100 miles from the nearest salt water. The Ringed seal is locally called "floe rat" and "gum seal." It is the smallest and least valuable of all and fairly common all round. The Harp seal is "seal," in the same way as cod is "fish." It has various local names, five among the French-Canadians alone but is specifically known

as the Greenland seal. The young, immediately after birth, have a fine white coat which makes them valuable. The herds are followed on a large scale at the end of the winter season, which is also the whelping season; hundreds of thousands are killed, females and young preponderating. They are still common along the east and south but diminishing steadily, especially in the St. Lawrence. The Bearded, or "Square-flipper," seal is rare in the St. Lawrence and on the Atlantic but commoner in Hudsonian waters. It is a large seal, eight feet long and bulky in proportion. The Grey or Horse-head seal runs up to about the same size occasionally and is one of the gamest animals that swims. It is rare on the Atlantic and not common anywhere on the St. Lawrence. The "Hoods" are the largest of all and the lions of the lot. They run up to 1,000 lb. and over and sometimes fourteen feet long. They are rare on the Atlantic and decreasing along the St. Lawrence, owing to the Newfoundland hunters. The Walrus, formerly abundant all round, is now rarely seen except in the far north, where he is fast decreasing.

Moose may feel their way in by the south-west to an increasing extent and might possibly be reinforced by the Alaskan variety. Red deer might possibly be induced to enter by the same way in fair numbers over a limited area. The woodland caribou is almost exterminated but might be re-suscitated. The barren-ground caribou is still plentiful in the north, where most of the herds appear to migrate in an immense ellipse, crossing from west to east, over the barrens, in the fall, to the Atlantic and then turning south and west through the woods in winter, till they reach their original starting-point near Hudson Bay in the spring. But this is not to be counted on. The herds divide, change direction and linger in different places. Their tame brother, the reindeer, is being introduced as the chief domestic animal of Eastern Labrador, with apparently every prospect of success. Beaver are fairly common and widely distributed in forested areas. Other rodents are frequent—squirrels, musk-rats, mice, voles, lemmings, hares and porcupines. There are two bats. Black bears are general; polars, in the north. Grizzlies have been traded at Fort Chimo in Ungava but they are probably all killed out. The lynx is common wherever there are woods. There are two wolves, arctic and timber, the latter now rare in the south.

The Labrador red fox is very common in the woods and the "white" or arctic fox in the barrens and further south on both coasts. The "cross," "silver," and "black" variations of course occur, as they naturally increase towards the northern limits of range. The "blue" is a seasonal change of the "white." The wolverine and otter are common. The skunk is only known in the south-west. The mink ranges through the southern third of the peninsula. The Labrador marten, or "sable," is a sub-species, generally distributed in the forested parts, like the weasel. The "fisher," or Pennant's marten, is much more local, ranging only between the "North Shore" and Mistassini.

From the St. Lawrence to the Barren Grounds three-fourths of the land has been burnt over since the white man came. The resultant loss of all forms of life may be imagined, especially when we remember that the fire often burns up the very soil itself, leaving nothing but rocks and black desolation. Still, there is plenty of fur and feather worth preserving. But nothing can save it unless conservation replaces the present reckless destruction.

Destruction

When rich virgin soil is first farmed it yields a maximum harvest for a minimum of human care. But presently it begins to fail and will fail altogether unless man return to it in one form some of the richness he expects to get from it in another. Now, exploited wild life fails even faster under wasteful treatment but, on the other hand, with hardly any of the trouble required for continuous farming, quickly recovers itself by being simply let alone. So when we consider how easily it can be preserved in Labrador and how beneficial its preservation is to all concerned, we can understand how the wanton destruction going on there is quite as idiotic as it is wrong.

Take "egging" as an example. The Indians, Eskimos and other beasts of prey merely preserved the balance of nature by the toll they used to take. No beast of prey, not even the white man, will destroy his own stock supply of food. But with the nineteenth century came the white-man market "eggers," systematically taking or destroying every egg in every place they visited. Halifax, Quebec and other towns were centres of the trade. The "eggers" increased in numbers and thoroughness till the eggs decreased in the more accessible spots below

paying quantities. But other egging still goes on unchecked. The game laws of the province of Quebec distinctly state: "It is forbidden to take nests or eggs of wild birds at any time." But the swarms of fishermen who come up the north shore of the St. Lawrence egg wherever they go. If they are only to stay in the same spot for a day or two, they gather all the eggs they can, put them into water and throw away every one that floats. Sometimes three, four, five or even ten times as many are thrown away as are kept and all those bird lives lost for nothing. Worse still, if the men are going to stay long enough they will often go round the nests and make sure of smashing every single egg. Then they come back in a few days and gather every single egg, because they know it has been laid in the meantime and must be fresh. When we remember how many thousands of men visit the shore and that the resident population eggs on its own account, at least as high up as the Pilgrims, only 100 miles from Quebec, we need not be prophets to foresee the inevitable end of all bird life when subjected to such a drain. And this is on the St. Lawrence, where there are laws and wardens and fewer fishermen. What about the Atlantic Labrador, where there are no laws, no wardens, many more fishermen and ruthless competitive egging between the residents and visitors? Of course, where people must egg or starve there is nothing more to be said. But this sort of egging is very limited, not enough to destroy the birds and the necessity for it will become less frequent as other sources of supply become available. It is the utterly wanton destruction that is the real trouble.

And it is just as bad with the birds as with the eggs. A schooner captain says, "Now, boys, here's your butcher's shop: help yourselves!" and this, remember, is in the brooding season. Not long ago the men from a vessel in Cross Harbour landed on an islet full of eiders and killed every single brooding mother. Such men have grown up to this and there is that amount of excuse for them. Besides, they ate the birds, though they destroyed the broods. Yet, as they always say "We don't know no law here," it may be suspected that they do know there really is one. These men do a partly excusable wrong. But what about those who ought to know better? In the summer of 1907 an American millionaire's yacht landed a party who shot as many brooding birds on St. Mary Island as they

chose and then left the bodies to rot and the broods to perish. That was, presumably, for sport. For the same kind of sport, motor boats cut circles round diving birds, drown them and let the bodies float away. The North Shore people have drowned myriads of moulting scoters in August; but they use the meat. Bestial forms of sport are many and vile. "C'est un plaisir superbe" was the description given by some voyageurs on exploring work who had spent the afternoon chasing young birds about the rocks and stamping them to death. Deer were literally hacked to pieces by construction gangs on new lines last summer. Dynamiting a stream is quite a common trick wherever it is safe to play it. Harbour seals are wantonly shot in deep fresh water where they cannot be recovered, much as seagulls are shot by blackguards from an ocean liner.

And the worst of it is that all this wanton destruction is not by any means confined to the ignorant or those who have been brought up to it. The men from the American yacht must have known better. So do those educated men from our own cities, who shoot out of season down the St. Lawrence and plead, quite falsely, that there is no game law below the Brandy Pots. It is, of course, well understood that a man can always shoot for necessary food. But this provision is shamelessly misused. Last summer, when a great employer of labour down the Gulf was telling where birds could be shot to the greatest advantage out of season and I was objecting that it was not clean sport, he said, "Oh, but Indians can shoot for food at any time—*and we're all Indians here!*" And what are we to think of a rich man who used caribou simply as targets for his new rifle and a scientific man who killed seventy-two in one morning, only to make a record? We need the true ideal of sport and an altogether new ideal of conservation and we need them very badly and very soon.

We have had our warnings. The great auk and the Labrador duck have both become utterly extinct within living memory. The Eskimo curlew is decreasing to the danger point and the Yellowlegs is following. The lobster fishing is being wastefully conducted along the St. Lawrence; so, indeed, are the other fisheries. Whales are diminishing: the Cape Charles and Hawke Harbour establishments are running but those at L'Anse au Loup and Seven Islands are not. The whole whaling industry is disappearing all over the world before the uncor-

trolled persecution of the new steam whalers. The walrus is exterminated everywhere in Labrador except in the north. The seals are diminishing. Every year the hunters are better supplied with better implements of butchery. The catch is numbered by the hundreds of thousands and this only for one fleet in one place at one season, when the Newfoundlanders come up the St. Lawrence at the end of the winter. The woodland caribou has been killed off to such an extent as to cause both Indians and wolves to die off with him. The barren-ground caribou is still plentiful, though decreasing. The dying out of so many Indians before the time of the Low and Eaton expedition of 1893–4 led to an increase of fur-bearing animals. But renewed, improved, increased and uncontrolled trapping has now reduced them below their former level. Hunting for the market seems to be going round in a vicious circle, always narrowing in on the quarry, which must ultimately be strangled to death. The white man comes in with better equipment, more systematic methods and often a "get-rich-and-get-out" idea that never entered a native head. The Indian has to go further afield. The white follows. Their prey shrinks back in diminishing numbers before them both. Prices go up. The hunt becomes keener, the animals fewer and farther off. Presently hunters and hunted will reach the far side of the utmost limits. And then traded, traders and trade will all disappear together. And it might so well be otherwise.

There is another point that should never be passed over. In these days the public conscience is beginning to realise that the objections to man's cruelty towards his other fellow-beings is something more than a fad or a fancy. And wanton slaughter is very apt to be accompanied by shameless cruelty. To kill off parents when the young are helpless. . . . But I have already given enough sickening details of this. The treatment of the adults is almost worse in many typical cases. An Indian will skin a hare alive and gloat over his quivering death-agonies. The excuse is, "white man have fun, Indian have fun too." And it is a valid excuse, from one point of view. When "there's nothing in caribou" except the value of the tongue, the tongue has been cut out of the living deer, whose only other value is considered to be the amusement afforded by his horrible fate. And fiendish cruelty like this is not confined to the outer wilds. When some civilised English-speaking bird-catchers get a bird

they do not want, they will deliberately wrench its bill apart, so that it must die of lingering starvation. Sometimes the cruelty is done to man himself. Not so many years ago some whalers secured a lot of walrus hides and tusks by having a whole herd of walrus wiped out, in spite of the fact that these animals were, at that very time, known to be the only food available for a neighbouring tribe of Eskimos. The Eskimos were starved to death, every soul among them, as the Government explorers found out. But Eskimos have no votes and never write to the papers; while walrus hides were booming in the markets of civilisation.

Things like these are not much spoken of. They very rarely appear in print. And when they are mentioned at all it is generally with an apology for introducing unpleasant details.

Conservation

All the sound reasons ever given for conserving other natural resources apply to the conservation of wild life—and with threefold power. When a spendthrift squanders his capital it is lost to him and his heirs; yet it goes somewhere else. When a nation allows any one kind of natural resource to be squandered it must suffer a real, positive loss; yet substitutes of another kind can generally be found. But when wild life is squandered it does not go elsewhere, like squandered money; it cannot possibly be replaced by any substitute, as some inorganic resources are: it is simply an absolute, dead loss, gone beyond even the hope of recall.

Now, we have seen verifiable facts enough to prove that Labrador, out of its total area of eleven Englands, is not likely to be advantageously exploitable over much more than the area of one England for other purposes than the growth and harvesting of wild life by land and water. How are these ten Englands to be brought under conservation, before it is too late, in the best interests of the five chief classes of people who are concerned already or will be soon? Of course, the same individual may belong to more than one class. I merely use these divisions to make sure of considering all sides of the question. The five great interests are those of: (1) Food; (2) Business; (3) the Indians and Eskimos; (4) Sport; and (5) the Zoophilists, by which I mean all people interested in wild-animal life, from zoologists to tourists.

1. *Food.*—The resident population is so sparse that there is not one person for every 20,000 acres; and most of these people live on the coast. Consequently, the vast interior could not be used for food supplies in any case. Besides, ever since the white man occupied the coast, the immediate hinterland, which used to be full of life, has become more and more barren. Fish is plentiful enough. A few small crops of common vegetables could be grown in many places and outside supplies are becoming more available. So the toll of birds and mammals taken by the present genuine residents for necessary food is not a menace, if taken in reason. In isolated places in the Gulf, like Harrington, the Provincial law might safely be relaxed, so as to allow the eggs of ducks and gulls to be taken up to June 15th and those of murres, auks and puffins up to the 15th. Flight birds might also be shot at any time on the outside capes and islands. There is a local unwritten law down there—"No guns inside after the first of June"—and it has been kept for twenty years. Similar relaxations might be allowed in other places, in genuine cases of necessity. But the egging and out-of-season slaughter done by people, resident or not, who are in touch with the outside world should be stopped absolutely. And the few walrus now required as food by the few out-living Eskimos should be strictly protected. Of course, killing for food under real stress of need at any time or place goes without saying. The real and spurious cases will soon be discriminated by any proper system.

2. *Business.*—Business is done in fish, whales, seals, fur, game, plumage and eggs. The fish are a problem apart. But it is worth noting that uncontrolled exploitation is beginning to affect even their countless numbers in certain places. Whales have always been exploited indiscriminately and their wide range outside of territorial waters adds to the difficulties of any regulation. But some seasonal and sanctuary protection is necessary to prevent their becoming extinct. The "white porpoise" could have its young protected; and whaling stations afford means of inspection and consequent control. The only chance at present is that when whales become too scarce to pay they are let alone and may revive a little. The seals can be protected locally and ought to be. The preponderance of females and young killed in the whelping season is a drain impossible for them to withstand under modern conditions of slaughter. The difficulty of policing large areas simultaneously might be compensated for by special

sanctuaries. The Americans are protecting their seals by restrictions on the numbers, ages and sex of those killed; and doing so successfully. The fur trade is open to the same sort of wise restriction, when necessary, to the protection of wild fur by the breeding of tame, as in the fox farms and to the benefits of sanctuaries. Marketable game, plumage and eggs can be regulated at outports and markets. And the extension of suitable laws to non-game animals, coupled with the establishment of sanctuaries, would soon improve conditions all round, especially in the interest of business itself. No one wants his business to be destroyed. But if Labrador is left without control indefinitely, every business dealing with the products of wild life will be obliged to play the suicidal game of competitive grab till the last source of supply is exhausted and capital, income and employment all go together.

3. *Indians and Eskimos.*—The Eskimos are few and mostly localised. The Indians stand to gain by anything that will keep the fur trade in full vigour, as they are mostly hunters and trappers. Restriction on the number of skins, if that should prove necessary and certainly on the sale of all poisons, could be made operative. Strychnine is said to kill animals eating the carcases even so far as to the seventh remove. Close seasons and sanctuaries are difficult to enforce with all Indians. But the registration of trappers, the enforcement of laws, the employment of Indians as guides for sportsmen, and other means, would have a salutary effect. The full-bloods, unfortunately, do not take kindly to guiding. Indians wishing to change their way of life or proving persistent lawbreakers might be hived in reserves with their wives and families. The reserves themselves would cost nothing, the Indians could find employment as other Indians have and the expense of establishing would be a bagatelle. As a matter of fact, in spite of all the bad bargains having always been on the Indian side when sales and treaties were made with the whites, there is enough money to the credit of the Indians in the hands of the Government to establish a dozen hives and keep the people in them as idle as drones on the mere interest of it. But good hunting grounds are better than good hives.

4. *Sport.*—Sport should have a great future in Labrador. Inland game birds, except ptarmigan, are the only kind of which there is never likely to be a great abundance, owing to the natural scarcity of their food. But, besides the big game on land

and game birds on the coast, there are some unusual forms of sport appealing to adventurous natures. Harpooning the little white whale by hand in a North Shore canoe or shooting the largest and gamest of all the seals—the great "hood"—also out of a canoe, requires enough skill and courage to make success its own reward. The extension and enforcement of proper game laws would benefit sport directly, while indirectly benefiting all the other interests.

5. *Zoophilists.*—The zoophilist class seems only in place as an afterthought. But I am convinced that it will soon become of at least equal importance with any other. All the people, from zoologists to tourists, who are drawn to such places by the attraction of seeing animal life in its own surroundings already form an immense class in every community. And it is a rapidly increasing class. Could we do posterity any greater injury than by destroying the ten Englands of glorious wild life in Labrador, just at the very time when our own and other publics are beginning to appreciate the value of the appeal which such haunts of Nature make to all the highest faculties of civilised man?

The way can be made clear by scientific study. The laws can be drawn up by any intelligent legislators and enforced quite as efficiently as other laws have been by the Mounted Police in the North-West. The expense will be small, the benefits great and widely felt. The only real hitch is the uninformed and therefore apathetic state of public opinion. If people only knew that Labrador contained a hundred Saguenays, wild zoos, Thousand Islands, fiords, palisades, sea mountains, cañons, great lakes and waterfalls, if they only knew that they could get the enjoyment of it for a song and make it an heirloom for no more trouble than letting it live, they might do all that is needed to-morrow. But they don't know. And the three Governments cannot do much without the support of public opinion. At present they do practically nothing. The Ungavan Labrador has neither organisation nor laws. The Newfoundland Labrador has organisation but no laws. And the Quebec Labrador has laws but no observance of them.

However, Quebec has laws, which are something, legislators who have made the laws and leaders who have introduced them. The trouble is that the public generally has no sense of responsibility in the matter of enforcement. It still has a hazy idea that Nature has an overflowing sanctuary of her own, somewhere or

other, which will fill up the gaps automatically. The result is that poaching is commonly regarded as a venial offence, poachers taken red-handed are rarely punished and willing ears are always lent to the cry that rich sportsmen are trying to take the bread out of the poor settler's mouth. The poor settler does not reflect that he himself and all other classes alike really have a common interest in the conservation of any wild life that does not conflict with legitimate human development. There is some just cause of complaint that the big-game reserves are hampering the peasants in parts of India and the settlers and natives in parts of Uganda. But no such complaint can be raised against the Laurentide National Park, so wisely established by the Quebec Government. The worst of it is that many of the richer people set the example in law-breaking. The numbers of big game allowed are exceeded, out-of-season shooting goes on and both out-of-season and forbidden game is sold in the markets and served at the dinner-tables of the very class who should be first in protecting it.

Partly because Quebec has taken the lead in legislation and partly because an ideal site is ready to hand under its jurisdiction, I would venture to suggest the immediate establishment of an absolute sanctuary for all wild birds and mammals along as much of the coast as possible on either side of Cape Whittle. The best place of all to keep is from Cape Whittle eastward to Cape Mekattina, 64 miles in a straight line by sea. The 45 miles from Cape Mekattina eastward to Shekatika Bay are probably the next best; and next, the 35 from Cape Whittle westward to Cloudberry Point. As there are 800 miles between Quebec and the Strait, I am only proposing to make from one-tenth to one-fifth of them into a sanctuary. And this part is the least fitted for other purposes, except sea-fishing, which would not be restricted at all, the least inhabited and the most likely to succeed as a sanctuary, especially for birds.

Cape Whittle is 550 miles below Quebec, 70 below Natashkwan, which is the last port of call for the mail boats and 50 below Kegashka, the last green spot along the shore. It faces Cape Gregory, near the Bay of Islands in Newfoundland, 130 miles across; and is almost as far from the north-east point of Anticosti. It is a great landmark for coasting vessels and for the seal herds as well. A refuge for seals is absolutely necessary to preserve their numbers and the business connected with them

Of course, I know there is a feeling that, if they are going to disappear, the best thing to do is to exploit them to the utmost in the meanwhile, so as to snatch every present advantage, regardless of consequences. But is this business, sense or conservation? Even if any restriction in the way of numbers, sex, age or season should be imposed on seal hunting, a small sanctuary cannot but be beneficial. While, if there is no other protection, a sanctuary is a *sine qua non.* It is possible that some protection might also be afforded to the whales that hug the shore.

The case of the birds is quite as strong and the chance of protection by this sanctuary much greater. With the exception of the limited egging and shooting for the necessary food of the few residents—the whole district of Mekattina contained only 213 people at the last census—not an egg nor a bird should be touched at all. The birds soon find out where they are well off and their increase will recruit the whole river and gulf. A few outlying bird sanctuaries should be established in connection with this one, which might be called the Harrington Sanctuary, as Harrington is a well-known telegraph station, a central point between Cape Whittle and Mekattina and it enjoys a name that can be easily pronounced. In the Gulf the Bird Rocks and Bonaventure Island to the south; one of the Mingan Islands, the Perroquets and Egg Island to the north; with the Pilgrims, up the river, above the Saguenay and off the South Shore, are the best. The Pilgrims, 700 miles from the Atlantic, are probably the furthest inland point in the world where the eider breeds. They would make an ideal seabird sanctuary. On the Atlantic Labrador there are plenty of suitable islands from which to choose two or three sanctuaries between Hamilton inlet and Ramah. The east coast of Hudson Bay is full of islands from which two corresponding sanctuaries might be selected, one in the neighbourhood of the Portland Promontory and the other in the south-east corner of James Bay.

There is the further question—affecting all migratory animals but especially birds—of making international agreements for their protection. There are precedents for this, both in the Old World and in the New. And so far as the United States are concerned there should be no great difficulty. True, they have set us some lamentable examples of wanton destruction. But they have also set us some noble examples of conservation.

And we have good friends at court in the members of the New York Zoological, the Audubon and other societies, in Mr. Roosevelt, himself an ardent conserver of wild life and in Mr. Bryce, who is an ex-president of the Alpine Club and a devoted lover of nature. Immediate steps should be taken to link our own bird sanctuaries with the splendid American chain of them which runs round the Gulf of Mexico and up the Atlantic coast to within easy reach of the boundary line. Corresponding international chains up the Mississippi and along the Pacific would be of immense benefit to all species and more particularly to those unfortunate ones which are forced to migrate down along the shore and back by the middle of the continent, thus running the deadly gauntlet both by land and sea.

Inland sanctuaries are more difficult to choose and manage. A deer sanctuary might answer near James Bay. Fur sanctuaries must also be in some fairly accessible places, on the seaward sides of the various heights-of-land and not too far in. The evergreen stretches of the Eastman river have several favourable spots. What is needed most is an immediate examination by a trained zoologist. The existing information should be brought together and carefully digested for him in advance. There are the Dominion, Provincial and Newfoundland official reports; the Hudson Bay Company, the Moravian missionaries; Dr. Robert Bell, Mr. A. P. Low, Mr. D. I. V. Eaton, Dr. Grenfell, Dr. Hare, Mr. Napoleon Comeau, not to mention previous writers, like Packard, McLean and Cartwright—a whole host of original authorities. But their work has never been thoroughly co-ordinated from a zoological point of view. A form of sanctuary suggested for the fur-bearing Yukon is well worth considering. It consists in opening and closing the country by alternate sections, like crops and fallow land in farming. The Indians have followed this method for generations, dividing the family hunting grounds into three parts, hunting each in rotation and always leaving enough to breed back the numbers. But the pressure of the grab-all policy from outside may become irresistible.

The one great point to remember is that there is no time to lose in beginning conservation by protecting every species in at least two separate localities.

A word as to the management and wardens. Two zoologists and twenty men afloat and the same number ashore could

probably do the whole work, in connection with local wardens. This may seem utterly ridiculous as a police force to patrol ten Englands and three thousand miles of sea. But look at what the Royal North-West Mounted Police have done over vast areas with a handful of men and what has been effected in Maine, New Brunswick and Ontario. Once the public understands the question and the governments mean business, the way of the transgressor will be so hard—between the wardens, zoologists and all the preventive machinery of modern administration—that it will no longer pay him to walk in it. Special precautions must be taken against that vilest of all inventions of diabolical ingenuity—the Maxim "silencer." No argument is needed to prove that silent firearms could not suit crime better if they were made expressly for it. The mere possession of any kind of "silencer" should constitute a most serious criminal offence. The right kind of warden will be forthcoming when he is really wanted and is properly backed up. I need not describe the wrong kind. We all know him, only too well.

Benefits

First, it cannot be denied that the constant breaking of the present law makes for bad citizenship and that the observance of law will make for good. Next, though it is often said that what Canada needs most is development and not conservation, I think no one will deny that conservation is the best and most certainly productive form of development in the case before us. Then I think we have here a really unique opportunity of effecting a reform that will unite and not divide all the legitimate interests concerned. What could appear to have less in common than electricity and sanctuaries? Yet electricity in Labrador requires water-power, which requires a steady flow, which requires a head-water forest, which in its turn is admirably fit to shelter wild life. Except for those who would selfishly and shortsightedly take all this wealth of wild life out of the world altogether, in one grasping generation, there is nobody who will not be the better for the change. I have talked with interested parties of every different kind and always found them agree that conservation is the only thing to do—provided, as they invariably add, that it is done "straight" and "the same for all."

Fourthly, a word as to sport. I have invoked the public conscience against wanton destruction and its inevitable accompaniment of cruelty. I know, further, that man is generally cruel and a bully towards other animals. And, as an extreme evolutionist, I believe all animals are alike in kind, however much they may differ in degree. But I don't think clean sport cruel. It does not add to the sum total of cruelty under present conditions. Wild animals shun pain and death as we do. But under Nature they never die what we call natural deaths. They starve or get killed. Moreover, town-bred humanitarians feel pain and death more than the simpler races of men, who in their turn feel it more than lower animals. A wild animal that has just escaped death will resume its occupation as if nothing had happened. The sportsman's clean kill is only an incident in the day's work, not anxiously apprehended like an operation or a battle. But pain and death are very real, all the same. So death should be inflicted as quickly as possible, even at the risk of losing the rest of one's bag. And, even beyond the reach of any laws, no animal should ever be killed in sport when its own death might entail the lingering death of its young. A sportsman who observes these rules instinctively and who never kills what he cannot get and use is not a cruel man. He certainly is a beast of prey. But so is the most delicate invalid woman when drinking a cup of beef tea. Sport has its use in the development of health and skill and courage. Its practice is one of life's eternal compromises. And the best thing we can do for it now is to make it clean. We have far too much of the other kind. I believe that the enforcement of laws and the establishment of sanctuaries will raise our sport to a higher plane, reduce the suffering now inflicted when killing for business and help in every way towards the conversion of the human into the humane. Besides, paradoxical as it may seem to some good people, the true sportsman has always proved to be one of the very best conservers of all wild life worth keeping. So there is a distinctly desirable benefit to be expected in this direction, as in every other.

Finally, I return to my zoophilists, a vast but formless class of people, both in and outside of the other classes mentioned and one which includes every man, woman and child with any fondness for wild life, from zoologist to tourists. There are higher considerations never to be forgotten. But let me first

press the point that there's money in the zoophilists—plenty of it. A gentleman in whom we can have the greatest confidence and who was not particularly inexpert at the subject has made an under-valuation to the extent of no less than 75 per cent. when trying to estimate the amount of money made by the transportation companies directly out of travel to "Nature" places for sport, study, scenery and other kinds of outing. There is money in it now, millions of it; and there is going to be much more money in it later on. Civilised town-dwelling men, women and children are turning more and more to wild Nature for a holiday. And their interest in Nature is widening and deepening in proportion. I do not say this as a rhetorical flourish. I have taken particular pains to find out the actual growth of this interest, which is shown in ways as comprehensive as educational curricula, picture books for children, all sorts of "Animal" works, "zoos," museums, lectures, periodicals and advertisements; and I find all facts pointing the same way. The president of one of the greatest publishers' associations in the world told me, without being asked, that the most marked and the steadiest development in the trade was in "Nature" books of every kind. And this reminds me of the countless readers who rarely hear the call of the wild themselves, except through word and picture but who would bitterly and justifiably resent the silencing of that call in the very places where it ought to be heard at its best.

Now, where can the call of wild Nature be heard to greater advantage than in Labrador, which is a land made on purpose to be the home of fur, fin and feather? And it is accessible, in the best of all possible ways—by sea. It is about equidistant from central Canada, England and the States—a wilderness park for all of them. Means of communication are multiplying fast. Even now, it would be possible, in a good steamer, to take a month's holiday from London to Labrador, spending twenty days on the coast and only ten at sea. I think we may be quite sure of such travel in the near future; that is, of course, if the travellers have a land of life, not death, to come to. And an excellent thing about it is that Labrador cannot be overrun and spoilt like what our American friends so aptly call a "pocket wilderness." Ten wild Englands, properly conserved, cannot be brought into the catalogue of common things quite so easily as all that! Besides, Labrador enjoys a double advantage in

being essentially a seaboard country. The visitor has the advantage of being able to see a great deal of it—and the finest parts, too—without getting out of touch with his movable base afloat. And the country itself has the corresponding advantage of being less liable to be turned into a commonplace summer resort by the whole monotonising apparatus of hotels and boarding houses and conventional "sights."

From the topmost heights of Evolution whole realms of Nature appear, infinitely surpassing all those of business, sport and tourist recreation; the theory of Evolution itself is the crowned brain of the entire Animal Kingdom. But I doubt whether, as yet, we fully realise that Labrador is absolutely unique in being the only stage on which the prologue and living pageant of Evolution can be seen together from a single panoramic point of view. The sea and sky are everywhere the same primeval elements. But no other country has so much primeval land to match them. Labrador is a miracle of youth and age combined. It is still growing out of the depths with the irresistible vigour of youth. But its titanic tablelands consist of those azoic rocks which form the very roots of all the other mountains in the world, which are so old, so immeasurably older than any others now standing on the surface of the globe, that their Laurentians alone have the real right to bear the title of "The Everlasting Hills." Being azoic these Laurentians are older than the first age when our remotest ancestors appeared in the earliest of animal forms, millions and millions of years ago. They are, in fact, the only part of the visible Earth which was present when Life itself was born. So here are the three great elemental characters, all together—the primal sea and sky and land—to act the azoic prologue. And here, too, for all mankind to glory in, is the whole pageant of animal life: from the weakest invertebrate forms, which link us with the illimitable past, to the mightiest developments of birds and mammals at the present day, the leviathan whales around us, the soaring eagles overhead and man himself—the culmination of them all—and especially migrating man, whose incoming myriads are linking us already with the most pregnant phases of the future. Where else are there so many intimate appeals both to the child and the philosopher? Where else, in all this world, are there any parts of the Creation more fit to exalt our visions and make us "Look, through Nature, up to Nature's God?"

Bibliography

General information is given in *Labrador; its Discovery, Exploration and Development, by W. G. Gosling: Toronto, Musson.* The Atlantic Labrador is dealt with by competent experts in *Labrador: the Country and the People, by W. T. Grenfell and Others: New York, The Macmillan Company*, 1910. This has several valuable chapters on the fauna. The Peninsula generally, the interior especially and the fauna incidentally, are dealt with in the reports of *A. P. Low* and *D.I.V. Eaton* to the *Geological Survey of Canada*, 1893-4-5. An excellent general paper on the country is *The Labrador Peninsula, by Robert Bell, in The Scottish Geographical Magazine* for July, 1895. The N. of the S.W. part is more particularly described in his *Recent Explorations to the South of Hudson Bay* in *The Geographical Journal* for July 1897. The Quebec Labrador is the subject of a recent Provincial report, *La Côte Nord du Saint-Laurent et le Labrador Canadien, par Eugène Rouillard: Quebec*, 1908—*Ministère de la Colonisation, des Mines et des Pêcheries.* An excellent account of animal life on the W. half of the Quebec Labrador is to be found in *Life and Sport on the North Shore, by Napoléon A. Comeau: Quebec*, 1909. The zoology of the Mammals, though not particularly in their Labrador habitat, is to be found in *Life-Histories of Northern Mammals, by Ernest Thompson-Seton: London, Constable*, 2 *Vols.*, 1910. The birds, similarly, in the *Catalogue of Canadian Birds, by John Macoun and James M. Macoun: Ottawa, Government Printing Bureau*, 1909. Some books about adjacent areas may be profitably consulted, like *Newfoundland and its Untrodden Ways, by John Guille Millais*, and American official publications, like the *Birds of New York, by Elon Howard Eaton: Albany, University of the State of New York*, 1910. No. 34 of the *New York Zoological Society Bulletin*, for June 1909, is a "Wild-life Preservation Number." The best general history and present-day summary of the world's fur trade is to be found in a recent German work, a genuine *Urquellengeschichte.* French and English translations will presumably appear in due course. The statistical tables are wonderfully complete. The illustrations are the least satisfactory feature. This book is—*Aus dem Reiche der Pelze, von Emil Brass: Berlin, im Verlage der Neuen Pelzwaren-Zeitung*, 1911.

REVIEWS

New Reduction Methods in Volumetric Analysis. By E. KNECHT and E. HIBBERT. [Pp. x + 108.] (Longmans, Green & Co. Price 3*s*. net.)

THE cult of the Text-Book has been so developed of late and such large numbers of students are little short of stupefied by this form of literature at the present day, that any book departing from current forms and showing evidence of independence is exceptionally welcome. That under review, in the first place, is to be commended for its originality of subject. Possibly the authors of so modest a volume realised that about a quarter of a century must elapse ere any new methods would find place in the established Text-Books and therefore decided to compile their own monograph upon the subject.

It is now more than nine years since the use of titanous chloride as a quantitative reducing agent was first recorded ; since then a considerable amount of work has been done showing that the use of this substance is of very general application.

The lack of suitable reducing agents unfortunately has prevented the development of volumetric methods of analysis based upon reduction processes. The authors are to be congratulated upon the part they have played in developing the methods of using titanous chloride in quantitative analysis, as well as for having brought together in practical form the records of work on this subject which have been scattered over a number of years and which have appeared in various journals. The collected information is presented in a form both clear and concise : chief interest, however, centres around the novelty of the methods rather than upon a real advance in accuracy or convenience in the estimations. Had a comparison been made of the results obtained by the usual methods with those obtained by the new methods described and had the authors discussed the advantage of these new methods over those usually adopted, the value of their book would have been greatly enhanced. As it is, there is little to convince the reader that the methods described are in any way superior to those generally adopted in the laboratory : for example, it might well be doubted whether any advantage would accrue from estimating the copper in the filtrate from a reduction of Fehling's solution by glucose rather than by proceeding in the usual way to reduce the collected precipitate of cuprous oxide and weigh or titrate the copper. Although similar arguments apply to other cases, there are many in which the use of titanous chloride becomes advantageous.

In carrying out some of the operations described a considerable amount of dexterity and practice is required but this should not deter those who would make use of the methods from essaying their hand, because consolation is to be found in the last paragraph of the Preface, where an assurance is given that second-year students are quite able to carry out the operations involved in the new methods of analysis.

J. V. EYRE.

The Revolutions of Civilisation. By W. M. FLINDERS PETRIE. [Pp. 131. Illustrated.] (Harper's Library of Living Thought: Harper & Brothers, London and New York, 1911. Price 2*s.* 6*d.*)

THIS is a pearl of price among books—one that should be pondered over by every thinking member of the community, as it carries a message, of vital importance to our race, conveyed with rare skill and a most unusual wealth of indisputable illustration. Prof. Petrie traces the rise and fall of no less than eight successive Egyptian civilisations and also of European civilisations, taking sculpture as the definite test of rise and fall. His conclusions are pessimistic in so far as they have any bearing on the question of our own future. The book consists largely of illustrations, many of which are most beautiful. No name is attached to most of the plates but those who are aware of the author's unrivalled skill as a photographer of Egyptian remains will recognise them as Prof. Petrie's handiwork.

The Chemistry and Testing of Cement. By CECIL H. DESCH, B.Sc., Ph.D. [Pp. xi + 267.] (London: Edward Arnold, 1911. Price 10*s.* 6*d.*)

THE author of this book has given in a compendious form an account of the chemistry and methods of testing—physical, chemical and mechanical—calcareous cements, appropriately leaving out of consideration those minor cementing materials, such as glue, casein preparations and the like, which have little in common with cements used for engineering and constructional purposes.

He includes plaster of Paris, ordinary lime mortar, hydraulic limes, Roman cement and the various cements made from blast furnace slag alone or with lime but properly devotes his chief attention to Portland cement as the most important member of its class. Besides the cements themselves, their raw materials are spoken of and a description is given of the usual methods of manufacture, whilst some space is allotted to concrete, including the reinforced material and the resistance of cement and concrete to the destructive influence of such agencies as sea-water, frost and certain classes of sewage. The book is illustrated by plates, some of which were prepared from photomicrographs; it also contains a number of useful tables giving numerical data of the mechanical and physical properties of cements.

It will be seen that the author's scheme is comprehensive and we may say at once that, although the book is of very modest dimensions, the ground has been well covered and the object in view has been satisfactorily achieved. The vexed question of the chemistry of cement has been handled with conspicuous ability, the doubtful or erroneous statements which have long disfigured this branch of work being rejected and the latest and best founded theories retained and explained. Although free from any partisan tendency, the author makes it abundantly plain that he holds the views of the most modern school of cement chemistry; on the balance of evidence we agree with his conclusions. In like manner, in the matter of testing he does not hesitate to condemn such obsolete methods as the determination of the weight per bushel and supports without reservation those later and better modes of examination, some of which, like the Le Chatelier test, have had to make their way against opposition not always disinterested. His remarks on the desirability of ensuring that concrete should be dense and as free as possible from voids, the spaces to be filled by cement being ascertained by actual measurement instead of left to chance by the adoption of some casual proportions without regard to the particular character of the

aggregate, are entirely to the point; equally useful is the insistence on the necessity for special care in this respect when reinforced concrete is prepared. We are aware that both engineers of standing and understanding and the various firms of contractors in this material with a reputation to lose spare no pains to ensure that this condition is fulfilled and the permanence of their buildings secured but it is idle to ignore the fact that careless work has been done and may be done again to the prejudice of a useful material and mode of construction; reiteration of the need for more than ordinary skill, caution and vigilance is of service as a warning and a guide.

We do not say that the book is without flaws or errors or that we agree with all the author's views and statements but we can say that the former are few and trivial and that the latter show a thorough scientific knowledge and an accuracy of thought and expression as rare as they are grateful to the serious student of a difficult and important subject. We congratulate Dr. Desch on the accomplishment of a sound and creditable piece of work.

BERTRAM BLOUNT.

The Progress of Physics, during thirty-three years (1875–1908). Four lectures delivered to the University of Calcutta during March 1908. By ARTHUR SCHUSTER, F.R.S. [Pp. 164.] (Cambridge: at the University Press, 1911. Price 3*s*. 6*d*. net.)

THIS is a quite fascinating little book—a veritable Jack Horner's Christmas Pie, in fact: if you put in your thumb, you can pull out a plum from almost every page; but the plums are not all ripe. It may be recommended strongly to those who desire to follow in broad outline the gradual development of modern electrical doctrine. Unfortunately, as in the case of too many books issued from the Cambridge Press of late, the sense is often greatly marred by false and unnecessary punctuation. Incidentally it may be noted that punctuation is one of those subjects that need to be dealt with by an International Conference.

In the opening pages of the book we learn that Clerk Maxwell introduced the fashion of doing experimental work into the Cambridge school in the early seventies—it is useful to be thus reminded of the modernity of the art of experimental inquiry in physics. Prof. Schuster does not question the educational value of systematic training in practical work but is doubtful whether it has had a material influence on the progress of science; he then offers the following noteworthy opinion: "The future investigator will no doubt ultimately save time if, at an early stage, he acquires a certain technical skill and becomes acquainted with physical methods; but otherwise the efforts of the teacher should be directed to stimulating his scientific activity rather than sending him through all manipulations which he might possibly have to perform." The argument is sound beyond question but where are the teachers who can so stimulate?—it is to be feared they are a rapidly diminishing quantity under modern conditions and that few teachers can cut themselves adrift from the drill-book.

"Can we trust to a mere bias—which may be a prejudice as well as a true instinctive guide—in deciding a question of a fundamental character?" is a question asked at the opening of Chapter II by the author to prepare his readers for an unorthodox statement regarding action at a distance. Our answer is—that we do, as a rule, in science as in ordinary life. From action at a distance we are led on to consider molecules of electricity—ions and ionisation—the determination of E/m and the introduction by Sir J. J. Thomson of his conception of detached

corpuscles, smaller than atoms, carrying a constant, necessary, minute, negative charge, the now familiar electron. The treatment of this chapter is specially interesting in view of the question asked at the beginning—the bias is clearly there. Prof. Schuster has swallowed Wilson's fog condensation method whole, as do most other electricians: yet there are among us those who remain not only unsatisfied but carry a large positive charge of doubt.

Radioactivity is the subject of Chapter III. In this the bias again peeps out and we meet with strange reasoning on the meaning of the word element.

Chapter IV opens with a pregnant sentence: "Difficulties cease to trouble us either when they are surmounted or when we have become accustomed to them. As soon as we observe a rare phenomenon, our brain starts working to find some plausible explanation. . . ." Confession is said to be good for the soul: in this sentence we have many "scientific" workers confessed! Terrestrial magnetism and finally the age of the earth come under consideration in this chapter. Lord Kelvin's conclusions are referred to but no mention is made of Prof. Perry's criticism thereon, though the justice of this, we believe, was admitted by Lord Kelvin.

Finally attention may be called to a paragraph on p. 122: "The progress of science is achieved by individual reasoning which cannot be transferred from one brain to another; and the average span of a lifetime must therefore be an important factor in the rate at which science progresses." There is profound truth in the remark and it is one that should lead us to attach more value than is perhaps customary to the individual labours of the competent few.

Introduction to Science. By J. ARTHUR THOMSON. Home University Library of Modern Knowledge. [Pp. 256.] (Williams & Norgate. Price 1*s.* net.)

THE title of this book is clearly a misnomer; it should be called—Gush on Science. The author shows that he has read widely and kept a commonplace book in which he has entered endless extracts from various writers; had he published these as extracts from the poets are often published (Tennysoniana, etc.), all would have been well; by intermingling them with his own remarks, however, he has produced a strange and most unsatisfactory hotchpotch which is scarcely calculated to inform those who are not aware what "Science" is and of no great use to those who are. As a psychological study—as throwing light on the writer's state of mind and as showing the impression that is being made on biologists by the results obtained by workers in other fields—the book is of considerable interest. But much of the "connective tissue" could well be spared. The book is "trippingly" written but we fear that much of it is properly described as only "half sound," if not as dangerously near to "penny-a-lining."

The author's failure to appreciate the definitions of science already given by men whose opinion does count is nowhere better displayed than on page 38, where we read, under "Science and Common-sense": "It is somewhat remarkable that several investigators of distinction have compared science to common-sense. . . . Huxley emphasised the idea that 'Science is nothing but trained and organised common-sense.' It seems to us that it would be nearer the truth to say that Science is sharply contrasted with common-sense," etc., etc. A man who either has or pretends to have such misconceptions as are contained in the paragraph under notice has no message to deliver on the meaning of science.

At p. 216 we read: "We have to remember also that the scientific spirit has been slowly driven home by positivism—that its formulations must be freed

from the vague and verbal." Readers of the book will scarcely gather this impression from its pages, as they are full of loose talk. An Introduction to Science should not be written lightly—to us it seems that Prof. Thomson has rushed at the task and has regarded it merely as a literary exercise.

Mendelism. By R. C. PUNNETT. [Pp. xiii+176, with Portrait of Mendel, six plates (five coloured) and many figures.] (Macmillan & Co., 1911. Price 5*s.* net.)

IN the book before us, Prof. Punnett has rewritten and much enlarged his well-known essay which gave so much satisfaction when it was first published in 1905. The book covers more ground than Mr. Doncaster's smaller work noticed in the July issue of this Journal. The account given of Mendelian doctrine by Mr. Punnett seems to us to be less simple and direct in statement than Mr. Doncaster's whenever the two overlap. It is interesting that Mr. Punnett sometimes uses common language in place of the technical terms used by Mr. Doncaster and that sometimes the reverse is the case—from which we draw the conclusion that the subject generally might well be translated almost entirely back from the Greek into straightforward English, at all events in books meant for popular consumption. If Mendelian studies are of the extreme value devotees would have us believe, it is all the more necessary that it should be made possible for ordinary mortals to understand what is written on the subject and that they should be able to read without the headache caused by the perpetual effort involved in giving meaning to the unfamiliar terms that are used.

The magnificent result achieved by Prof. Biffen under the guidance of Mendelian principles, in raising a strain of wheat which is not only "hard" and "rust-proof" but also gives heavy crops under English conditions, is sufficient in itself to recommend the subject as one of practical importance. The infinitely difficult task now before us is to raise a white race that can withstand the coming onset of the coloured peoples.

If Messrs. Doncaster and Punnett could combine forces (perhaps calling in Mr. Whetham's aid) and produce a work that ordinary intelligent folk could read, in which the vital importance of the issues dealt with by Mendelism was duly set forth and emphasised, they would be rendering enormous service to the public. The future of our race is clearly the stake to be played for.

Perhaps one impression that remains after reading the book may be mentioned —it is that modesty is apparently a word not yet included in the Mendelian vocabulary. The cock-sureness with which conclusions are formulated is very striking—so striking that it may raise doubt in the minds of those outside the ring : the charitable interpretation is, of course, that it is but a sign of juvenile enthusiasm.

A Treatise on Practical Light. By R. S. CLAY, B.A., D.Sc. [Pp. 519.] (Macmillan & Co. Price 10*s.* 6*d.* net.)

THIS is an extended edition of the work previously published by the author entitled *Practical Exercises in Light.* It now contains over 500 pages and more than 400 diagrams and certainly presents an exceptionally complete record of experiments on optics. The author explains that his intention has been to give special attention to problems of practical and industrial importance. Nevertheless, the title sounds a little singular.

The first chapter is devoted to simple experiments with pins. We then meet the standard series of exercises on simple lenses, mirrors and optical instruments and more advanced work on the optical bench, compound lenses, gratings and polarised light follows. Two matters dealt with in unusual detail are interferometers and the compound microscope. There are also chapters devoted respectively to photometry, colour measurement and vision.

The chapter on photometry might well be extended somewhat in a subsequent edition. A novice would probably experience some difficulty in understanding the process from the brief directions given and some of the apparatus shown seems somewhat primitive. One would like to see some reference to the industrial side of this subject, including the measurement of illumination. The chapters on colour would gain in value if an account of some important physiological phenomena (such as the Purkinje effect and the influence of adaptation to strong and weak light) were included. (On p. 436 the author surely means to say that the fovea centralis contains *cones* only and *no rods*, not "rods only and no cones.")

However, in a work covering such an exceptionally wide field it is naturally difficult to enter deeply into details, important though some of them may be. The work will doubtless prove very serviceable to teachers of optics and advanced students, who will find in it many suggestions for new experiments.

J. S. D.

Illumination, its Distribution and Measurement. By A. P. TROTTER. [Pp. xvii + 292.] (Macmillan & Co. Price 8*s.* 6*d.* net.)

IN his introduction to this work Mr. Trotter recalls the words of Count Rumford, who, in 1789, expressed his wonder that so important an art as illumination should suffer neglect. At that time methods of testing and even the necessary technical terms were wanting. Bouguer (whom Mr. Trotter styles the "Father of Photometry"), followed by other physiologists and physicists, had studied the measurement of light; but for many years the subject was regarded more as a perplexing and fascinating field for study than as a process of any industrial value.

Mr. Trotter's well-known paper before the Institution of Civil Engineers was read just 103 years after Rumford's remarks. In the meantime the status of photometry had improved materially; the subsequent rapid progress in artificial illuminants and the increased attention paid to their practical utilisation have given it an added importance. In 1908 Mr. Trotter's paper began to appear in a rewritten and expanded form in *The Illuminating Engineer*. It has since been once more revised and brought up to date and now appears as a treatise on modern photometry.

The author's long identification with photometry and the study of scientific lighting fits him for this task. The early part of the book is devoted mainly to units, standards and terms used in connection with illumination. The chapters on photometric apparatus following are perhaps as valuable as any in the book, much of the advice given being clearly the result of personal experience. Useful hints are given regarding the arrangement of photometric benches and scales, the avoidance of stray light, etc.; and a summary is presented of the important work done during the last few years in connection with the distribution of light from modern lamps and the determination of mean spherical candle-power.

The author makes some illuminating comments on the nature of errors in photometry and he discharges satisfactorily the difficult task of dealing with the

perplexing subject of colour-photometry. A separate chapter is devoted to illumination-photometers. This is a good feature, for it is surprising how little is generally known of the convenience and simplicity of modern methods of measuring illumination. The remaining chapters present the results of some actual measurements in the streets of London and trace the development of methods of distributing the light by means of prismatic glass globes and reflectors.

Mr. Trotter's book should serve to satisfy a distinct want. It may be added that the style is lucid and that the diagrams (over 200 in number) and printing are excellent. At the conclusion of the book will be found a very serviceable bibliography, in which the references previously given at the foot of the pages are collected together in alphabetical order.

J. S. D.

Astronomy. By ARTHUR R. HINKS. [Pp. vi + 256.] (Home University Library of Modern Knowledge : Williams & Norgate. Price 1*s.* net.)

THIS is an altogether model book, one that should be on all railway bookstalls and that should be read by intelligent travellers who desire to gain some idea of the nature of the fascinating problems which astronomers are seeking to solve. It is written in an exceptionally clear and flowing style and in a critical spirit, so that the reader feels that he is under the safe guidance of a man imbued with the truly scientific habit of mind. It is just the kind of book that is required to lead intelligent general readers who are in no way specialists to take an interest in scientific work.

A Text-book of Inorganic Chemistry. By GEORGE SENTER, D.Sc., Ph.D. [Pp. x+583.] (Methuen & Co., London, 1911. Price 6*s.* 6*d.*)

LORD ROSEBERY initiated recently a discussion on the value of the books that are stored in our libraries, contending that most of them are to be regarded as dead and not worth shelf room. There is certainly a large class that ought never to have been born ; the book before us is one of them ; it is difficult to understand how a person gifted with the knowledge and intelligence to compile such a treatise can have been led to waste his energies in composing it. There was a time when publishers had souls and took some care in selecting books : now it would seem that commercial considerations alone count : each publisher must have his series and it matters little whether or no a book is a mere repetition of the errors of those that have gone before it, without any feature of novelty. Obviously Dr. Senter *has undertaken to write a cram book*, as he admits in the preface that "The book is designed for use in University, Technical Institute and other general classes on the subject and contains all that is usually included in a B.Sc. course." Poor B.Sc. candidates ! Little wonder that those marked with these letters are too often labelled thereby as incompetent chemists ; any one who knows what chemistry is in practice, who will turn over the pages of the book, must realise that the student brought up on such pabulum will have sacrificed all power of independent thought in acquiring the merest simulachre of knowledge.

The work is a scissors-and-paste affair—a text-book of the old cookery-book recipe type has been cut up and sections irrelevantly introduced such as are found in the more modern treatises on so-called Physical Chemistry. In other words, the dogmas of the ionic dissociation school are interlarded with conventional

tit-bits of information which commonly pass as chemistry in academic circles, though nowhere in real life.

If any one wish to know what our examination system is doing to degrade science—if any one wish to understand why it is that reasoning power is a minus quantity among our students and science decadent—let him take this book and read, mark, learn and inwardly digest its contents ; let him ask himself how in Heaven's name the student can acquire any knowledge of method from its pages, anything worthy to be called scientific education. Yet it is in no wise worse than its many predecessors of the same type—it is well printed, the language is clear, it contains a fund of information of a kind. The illustrations are few ; several are of indifferent merit and such as to encourage slovenly work. Thus in fig. 6, on p. 38, illustrating the production of water on burning hydrogen, this gas is depicted as evolved from an ordinary Woulf's bottle provided with a thistle funnel instead of from one permitting of the regulation of the rate at which the gas is produced and delivered, such as must be used in carrying out the demonstration properly. It is customary to insist on round bends being made in glass tubes: in the apparatus figured on p. 290, used in demonstrating the production of sulphuric acid, the tubes are drawn with sharp bends. Such slips are clear proof that the author is thinking of the examination hall and not of what is right and proper in a laboratory. On p. 56 a figure is given illustrating the arrangement used by Dumas and Stas *in 1843* in determining the composition of water—in this not only are some parts not to the original scale but the bulb containing the copper oxide is shown to be provided with a ground-in glass tap of modern pattern and as heated by a Bunsen burner ! Such historical veracity is inexcusable.

As was to be expected, the author is most at home in the physical sections of the book—though these suffer not a little from the too frequent dogmatic methods of statement characteristic of the modern obscurantist school of which he is a disciple. Strange examples are to be met with in these sections—thus :—

"*The atomic weight of an element is therefore the smallest amount of it which occurs in a molecule of one of its compounds, referred to the atom of hydrogen as unity* (*strictly* 1·008)."

Such is the logic of chemists : unity 1·008 not 1, thanks to Landolt, Ostwald and Co. and the settlement faked by votes, not by intelligence, to impose an irrational, unscientifically chosen set of values upon the profession under the guise of international "agreement."

"Applying Avogardo's hypothesis, that one molecule occupies two unit volumes, we have

2 vols nitrogen + 6 vols hydrogen + 4 vols ammonia.
N_2 $3H_2$ $2NH_3$.

Since the four volumes of ammonia must also contain two molecules, it follows at once that the formula for ammonia is NH_3. In this case it is necessary to know the number of atoms in the molecules of hydrogen and of nitrogen and it must be remembered that the molecules of all substances do not contain two atoms."

Avogardo would have difficulty in recognising his work in such guise !

The chemical sections are in many cases extraordinarily weak—consisting for the most part of brief statements that can be memorised and reproduced in an examination paper. The section on Oxides and Oxyacids of nitrogen may be taken as an example ; in this, in dealing with the interaction of nitric acid and

metals, not one word is said of the work done by Russell, Veley and others showing the part that is played by nitrous compounds. Under "Modern methods of writing complicated equations," printed in Clarendon type, we have the following choice morsels:

"An equation such as that representing the action of nitric acid on copper can readily be written in stages. In such a case it is perhaps simplest to assume that the oxygen of the nitric acid is used up in converting the metal to oxide, the latter then uniting with more nitric acid to form the nitrate. As a preliminary to writing the equation, the stage to which the nitric acid is reduced must be determined experimentally and the formula of the nitrate must be known. The steps in the action of nitric acid on copper are as follows:—

(1) $2HNO_3 = H_2O + 2NO + 3O$.
(2) $3Cu + 3O = 3CuO$.
(3) $3CuO + 6HNO_3 = 3Cu(NO_3)_2 + 3H_2O$.

Adding up—

$3Cu + 8HNO_3 = 3Cu(NO_3)_2 + 2NO + 4H_2O$."

What has the chemist to do with what "is perhaps simplest to assume"? His duty is to ascertain, as far as possible, what actually happens; it is pretty certain that the picture here painted for us is purely imaginary.

Of late years, we have had "Reform methods" introduced of teaching modern languages and even practical methods of teaching the classics; in fact, the methods of teaching almost every subject have been under consideration. Chemistry almost alone—to judge from the text-books, of which there is an unceasing flow—remains unreformed and there is little or no relation between the work of the schools as tested in examinations and the practice of the workshop and factory: the first lesson learnt by the B.Sc. on his entrance into life, as a rule, is that he has wasted precious years in useless labour; too often his mind has been so unhinged that he fails to recover himself and remains but a laboratory drone for the rest of his career. These are the things University Commissioners should consider—not the provision of examination halls and pálatial buildings for the preservation of red tape. To stem the ruinous tide of democracy and socialism, we need absolute free trade in University teaching: above all things, at the present moment, we need thinking men—not mumblers of shibboleths.

H. E. A.

Treatise on Dynamics. By A. GRAY, LL.D., F.R.S., and J. G. GRAY, D.Sc. [Pp. xvi + 626] (Macmillan & Co. Price 10*s*. net.)

THE previously published works of Prof. Andrew Gray, characterised as they are by thoroughness and explicitness and imbued with the scientific spirit, lead us to commence the perusal of this work with not a little expectancy and it may be said at once that we have not been disappointed. The careful enunciation of first principles, the neat mathematical analysis and the applications of the principles discussed to the solution of a large number of problems of scientific and practical interest, make the work one of real value to students of pure science and engineering. In the opening chapter, the kinematics of a moving point are dealt with, commencing with definitions and the conceptions the writers have of speed and velocity; we are tempted to dwell upon the importance of the distinction made by the authors between an instant and an interval of time, as this difference is not only of importance in the study of dynamics but also helps to solve the famous paradox of Zeno and to a clearer understanding of the

conception of continuous change emphasised by certain modern philosophers. After curves of velocity for motion in two dimensions, including the hodograph and accelerations, have been dealt with, a number of examples of rectilinear motion are considered, one being a neat investigation of the motion of the crosshead of a steam engine relative to the fixed frame and to the crank pin. A very interesting and important fundamental theorem relating to moving axes is then enunciated and proved ; this is applied afterwards to the solution of gyrostatic problems of interest to the scientist and to the engineer. The remainder of this chapter deals with more difficult acceleration problems, three-dimensional motion and harmonic motion. In Chapter II. dynamical principles are discussed, the laws of motion and work and activity being fully considered. This chapter may be specially commended to all serious students and to teachers of dynamics, as the discussion of the laws of motion is suggestive and illuminative and the results obtained both for bodies of constant and variable mass are of great importance. In the next chapter the motion of a particle in a resisting medium is considered, a number of important examples being solved. James Bernouilli's solution of the time of quickest descent in a given field of force is reproduced and is followed by a general discussion of brachistochrones and the use of the calculus of variations in their solution. The use of the term "conservative forces" without definition is hardly fair to readers ; later in the volume the authors seem to recognise this, as the term is defined in Chapter X. Chapter IV. deals almost entirely with astronomical problems, the free motion of a particle under a force directed to a fixed point being fully discussed. A short chapter on the motion of rigid bodies, in which a number of important theorems are given, is followed by an interesting chapter on the application of dynamical principles. On account of its suggestiveness as well as for the importance of the results deduced, the first part of this chapter will well repay perusal by engineers directly interested in traction : we know of no other English work in which the dynamics of the subject is so well treated. On p. 351, on which the problem of an engine running round a curve is considered, there is a slight error, the 2b in the figure corresponding to b in the text. The gyrostatic effect of the wheels of the locomotive as the engine runs round the curve is dealt with later, though it might well have been considered here as a direct application of the important principle mentioned above. The possibility of oscillations causing an engine to leave the track at speeds below the so-called critical speed is considered but the important effect of the unbalanced masses on the crank shaft is not dealt with ; an application of the principles of the previous chapter will, however, enable the reader to obtain the magnitude of the overturning couple due to these masses. In this chapter, the compound pendulum and forced vibrations are dealt with and consideration is given to the pile driver and ballistics. After a short chapter on the rotation of rigid bodies, tops and gyrostats are comprehensively treated ; the interest and importance of this chapter will be obvious when it is said that in addition to the analogy of motion of an electron in a magnetic field to the motion of a gyroscope being considered, astronomical precession, the Schlick gyrostatic controller of rolling ships, the barygyroscope for demonstrating the earth's rotations, the Brennan mono-rail, the gyrostatic action of revolving masses in steamers and the gyrostatic couples on locomotives and carriages when running round curves or running over a track with changing gradient are all considered. In Chapter X. general dynamical principles are dealt with, Lagrange's and Appell's equations and Hamilton's transformation of the equations of Lagrange being elegantly treated. A number of applications of these equations is also given. The work

concludes with a chapter entitled "Statics," the subjects dealt with—including the theory of the suspension-bridge chain and the transmission of power by belts—being not altogether consistent with the general aim of the book or with the title of the chapter in which they are found; nevertheless this chapter will be perused with profit by engineering students. It will be evident that this work is of much more than academic interest; as problems of great practical importance are dealt with, it should appeal both to the mathematician and to the man of affairs; in addition, the large number of well-chosen examples in the text and at the end of each chapter will be found useful by teachers and students. The book fills a gap that has long existed between the many elementary works on dynamics and the classical work of Routh; it should find a place not only among the treasured few volumes of the student but also among the books of reference of the practising engineer.

F. C. L.

FALLACIES IN THE REPORT OF THE ROYAL COMMISSION ON TUBERCULOSIS

By RALPH VINCENT, M.D., M.R.C.P.
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The Final Report of the Royal Commission on Human and Animal Tuberculosis has almost completely failed to carry conviction to those who have studied the subject. There are few pathologists who express anything but a very modified approval of the Report, the usual view being that the Commissioners have throughout failed to grapple with their subject in the logical and thorough manner essential in an inquiry fraught with such consequences to the public health. The general community, unaware apparently of the reception that has been accorded to the Report by scientific men, is disposed to regard the questions discussed by the Commissioners as finally settled: their decisive statements and the vigorous terms in which their conclusions are formulated have been extensively reproduced in the Press and have done much to mislead both the medical profession and the general public. In these circumstances it would appear to be a duty to indicate the fallacies inherent in the Report and the grave dangers involved in the acceptance of the views so strongly advocated by the Commissioners.

It is necessary, however, to distinguish between the Report and the scientific work carried out by the various experts engaged on behalf of the Commissioners. The results of the scientific work are recorded in numerous appendices; these constitute a mine of information for the serious student of the subject. The Final Report of the Royal Commission, in the writer's opinion, however, wholly fails to do justice to the scientific investigations on which it purports to be based. In fact, the conclusions of the Royal Commissioners are widely divorced from the scientific observations. The whole trend of the scientific observations appears to run one way, the

whole trend of the Report the other. In one instance of critical importance the statement in the Report is diametrically opposed to the statement of the expert engaged in the investigation of the question.

In the Second Interim Report (page 7) the following passage occurs:

"The Effects of Introducing the Bacillus of Bovine Tuberculosis into the Body of the Bovine Animal

"We have adopted two chief methods of introduction: (1) *Feeding*, (2) *Injection into the tissues.* We considered the desirability of making use of a third method, viz. the introduction of the bacillus into the lungs by the air-passages; but decided not to adopt this method, at least in the first instance."

This passage is eminently characteristic of the Report. It is generally agreed that the infection of tuberculosis commonly gains entrance to the human body by way of the respiratory tract. The Commissioners at the very beginning of their investigation immediately proceed to put out of court inhalation experiments; and, so far as can be ascertained from all the subsequent publications, no inhalation experiments were carried out at any time.

Even more characteristic of the Report is the summary manner in which the Commissioners deal with this critical point. *Why* did they decide "not to adopt this method"? They give no reasons. It is sufficient for them to state their decision. This assumption of infallibility is a distinguishing and amusing feature of the Report and is probably the explanation of the failure of the Royal Commissioners to appreciate the real nature of the scientific problems they were called upon to study.

Throughout the Report the Commissioners write as if they were obsessed by the idea that they have been appointed to bring in a verdict that a good deal of the tuberculosis prevalent in man is conveyed to him by means of cows' milk. It is a conclusion wholly divorced from reality and it would be strongly and earnestly contested by nearly every practical physician who has studied the incidence of the disease. The fact is that the endeavour to trace any considerable amount of tubercular disease in man to infection by means of cows' milk has completely failed; those who have made the attempt have pointed

out very clearly the results of their observations. Emmett Holt, of New York, has made a special study of this question and two passages may be quoted from his book, *Diseases of Infancy and Childhood*, since they serve admirably to illustrate the attitude of expert physicians and pathologists on this question.

"Much stress has been laid upon tuberculous milk as a means by which children are affected. There is little pathological support to be found for the view that children often contract the disease in this way. In 119 autopsies upon tubercular children, chiefly infants, there was not found one in which the most advanced and therefore presumably the primary lesion was in the intestines or stomach. In 127 autopsies, also upon tubercular infants, Northrup found the most advanced lesions in the intestines in but a single case. While infection from milk is possible, it is certainly extremely infrequent. In my own autopsies, intestinal lesions have been found only in marked cases of generalised tuberculosis."

"Near a large American city was a fancy stock farm of registered Jersey cows, which supplied milk for table-use and infant-feeding to a large number of families in the wealthiest part of the city for a period of over ten years. At the end of that time the tuberculosis test was used for the first time and 45 per cent. of the cows were found to be tuberculous and were killed by order of the State Board of Health.

"The diagnosis was confirmed by autopsies upon the animals in every case. An investigation was instituted among the children who had been fed upon this milk but in only one case of many hundreds could it be learned that tuberculosis had developed and in this instance it was by no means established that the milk had been the source of infection.

"It should be stated that this was before the days of sterilising milk for infant-feeding. Besides the families who took the milk in the manner mentioned, the employés at the farm were accustomed to drink the skimmed milk in large quantities daily as a beverage. Many of them continued to do this for years and yet not one of them developed tuberculosis."

The attitude of the Royal Commissioners may be illustrated by two quotations. It is significant that these hopelessly inconsistent statements occur within a few lines of one another. One is on page 27 of the Second Interim Report; the other is on page 28. It should be explained that the paragraph on page 28 refers to organs which had been removed *post mortem* from children who had died from tuberculosis.

QUOTATION FROM PAGE 27

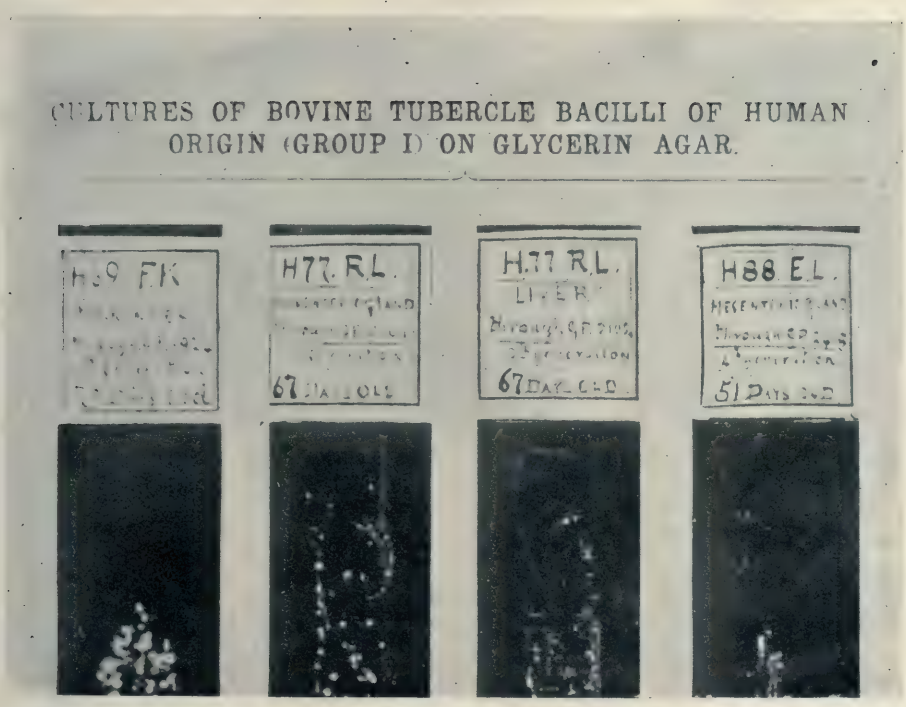
In the first class we place the bacillus of bovine tuberculosis and the bacillus of Group I. of human tuberculosis. These exhibit the same range of growth on various media and the same correlated variations in microscopical features. They are in all respects one and the same.

QUOTATION FROM PAGE 28

In three of the cases in which we obtained tuberculous mesenteric glands we were able to study not only the mesenteric glands themselves but also tuberculous lungs and other organs; and the results obtained from these other tuberculous organs were identical with the results obtained from the mesenteric glands; the tuberculosis in all the affected organs was the same bovine tuberculosis, one form only of bacillus having been found. The bacilli of bovine source had not only attacked the mesenteric glands to which they had been brought directly by absorption from the alimentary canal but had also invaded distant organs, being carried thither by the blood-stream or the lymphatics. We may conclude beyond doubt that the whole disease was caused by the same bovine tubercle bacilli: the children died of bovine tuberculosis caused by bacilli introduced by way of the alimentary canal and probably conveyed through cows' milk.

Before discussing these two passages it is necessary to emphasise one point. The word *bovine* means nothing whatever in regard to the *source of infection.* The term "bovine bacillus" is a term indicating that the bacillus belongs to a type possessing special features in regard to its cultural characteristics and its relative degree of virulence. The word "bovine" is a laboratory colloquialism used to signify that variety of bacillus which attacks human beings and also attacks bovines to distinguish it from that variety of bacillus which is, as a rule, not virulent enough to attack bovines. A physician, for example, observes in his laboratory that the tubercle bacillus infecting a particular child is of the "bovine" type. Does he conclude that the child got it from the cow? Of course he does not. He concludes nothing whatever in regard to the *source of infection.*

Do the Royal Commissioners disagree with this? On the contrary, they entirely agree with it. When they find the bovine bacillus, can they distinguish it from Group I. of human tuberculosis? Can they say this bacillus came from a cow and *not* from a human being? Of course they cannot and on page 27



Photograph of a portion of page 532, Final Report of Royal Commission on Tuberculosis, Part II. Appendix, vol. i., showing that the organisms described in the Report as "bacilli of bovine source" are described by the scientific investigator attached to the Commission as "bovine tubercle bacilli of human origin."

they tell the reader so in plain emphatic words which leave no room for misunderstanding.

Now we turn to what must be one of the most extraordinary passages that has ever appeared in the report of a scientific investigation—the passage quoted from page 28. Let us analyse the statements. Firstly, the Commissioners state that they have found bovine tuberculosis in human beings. This is absurd. Bovine tuberculosis is tuberculosis in bovines. Human tuberculosis is tuberculosis in human beings. What they mean is that they have found "bovine" bacilli in cases of human tuberculosis.

In a flash they ring the changes. The next sentence begins with the words—"the bacilli *of bovine source.*" In other words, they beg the whole question; they determine the very questions they were appointed to decide not by argument, not by investigation but by word-jugglery of the worst order. What right have they to use the words "bacilli of bovine source"? They have no right and, if words mean anything (*vide* the quotation from page 27), they know they have no right.

This Report purports to be based upon the investigations carried out by their technical experts. The scientific work dealing with the nature of the bacillus found in these cases is discussed in a volume by Dr. A. Stanley Griffith: *Investigation of Viruses obtained from Cases of Human Tuberculosis (other than Lupus).*" In this volume Dr. Griffith published photographs of the cultures obtained. What is his description of the organisms referred to by the Commissioners? Does he describe them as "bacilli of bovine source"? Dr. Griffith's description (taken from page 532) is "Cultures of Bovine Tubercle Bacilli of Human Origin (Group I.) on Glycerin Agar." Lest there should be any question of misquotation the writer has photographed that portion of the page referred to.

By suppressing the words "of human origin" and substituting for them the words "of bovine source" the Commissioners manage to maintain the argument. But it must not be forgotten that the whole passage depends upon this substitution of words. Employ the proper description as it is employed by Dr. Griffith and the whole argument simply falls to pieces.

Having thus extricated themselves from the difficulties of demonstrating what cannot be demonstrated, the Commissioners proceed to their predestined and prearranged conclusion.

In the Final Report a list is published of twenty-nine cases

of "primary abdominal tuberculosis." The moment one begins to study the details difficulties arise. Six of the cases died of generalised tuberculosis. Three others died of tubercular meningitis. So far as the writer has been able to ascertain, there is simply no evidence in support of the statement that these cases of tuberculosis in children were primarily abdominal. But we must pass on.

According to the Report the crux of the case as showing the conveyance of tubercular disease by means of cows' milk is to be found in these twenty-nine cases. They are classified as follows: In *fourteen* cases the bacillus was of the "bovine" type; in *thirteen* cases the bacillus was of the "human" type; in *two* cases both "bovine" and "human" bacilli were found. The precision of the classification is quite uncanny. Not a single case in which it is difficult to classify the bacillus found! Not one case in which the bacillus behaves in some respects as a "human" type in other respects as a "bovine" type! "C'est magnifique, mais ce n'est pas la guerre."

Omitting the two cases of mixed infection, we have to deal with twenty-seven cases. In the Report itself it is claimed that in these cases of "primary abdominal tuberculosis" in children the "bovine" bacillus is rather more common than the "human" bacillus—a strange basis indeed for an argument directed all the time to proving that cows' milk is the only likely means by which the bovine bacillus can reach the child.

To define the situation in actual terms, let us take a single child from the list of fourteen cases. She is a little girl aged eight years and her hypothetical name shall be Emily Johnson. Emily Johnson is riddled with tuberculosis ("bovine" bacillus). She is coughing up or otherwise expelling from her body millions of bovine bacilli. Her little sister sleeps in the same room with her. Her constant companion in the daytime is Tommy Smith.

The doctor examines the sputum or other *materies morbi*. If he find the "human" bacillus he will be alive to the dangers to which the companions of Emily Johnson are exposed; but if he find the "bovine" bacillus (the more virulent type, be it noted) he will inform those concerned that there is really no danger. Tommy Smith will never contract tuberculosis from Emily Johnson. But the reader is incredulous. Is it a fact that the Commissioners state that children suffering from tuberculosis are not dangerous to their companions provided that the bacillus

attacking them be of the "bovine" type? No; it is not a fact. This statement is never made *explicitly*; the grievous wrong is that this statement is made *implicitly* in almost every other line of the Report.

But we may study the methods of the Royal Commission a little further. Tommy Smith in fact does contract the disease. At a later stage his case comes under the observation of the Commissioners. They find the "bovine" bacillus and, straightway, the case joins the list of cases proving the transmission of the disease by means of cows' milk. Unfortunately, the parents of Tommy Smith have been for many years past in very poor circumstances. They have scarcely known what fresh milk is. They have generally used condensed milk and when more than usually pressed they have used condensed milk made from skimmed milk. But what does that matter? Everything is grist to the mill of the Royal Commissioners. And so they proceed to their final conclusions, displaying a wealth of assertion that makes one gasp. The following quotations are taken from page 39 of the Final Report:

"The evidence which we have accumulated goes to demonstrate that a considerable amount of the tuberculosis of childhood is to be ascribed to infection with bacilli of the bovine type transmitted to children in meals consisting largely of the milk of the cow."

". . . The potency of tuberculous cows' milk in the causation of the tuberculosis of infancy and childhood is clearly demonstrated. . . ."

These statements are simply untrue. There is not a line of evidence to show that the children whose cases were investigated by the Commissioners ever received cows' milk—tuberculous or otherwise.

SCIENCE AND THE AVERAGE BOY

By ARCHER VASSALL, M.A., F.Z.S.
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The teaching of science at public schools is now at a transitional stage when discussion and constructive proposals especially may do much to promote its future efficiency.

The scientific training given at these schools was limited, in the past, to such as might be obtained from attending lecture demonstrations. The boys came to the lectures grouped in forms according to their proficiency in subjects other than science and without any regard being paid to their intellectual interests or scientific aptitude. The subject of the lecture was usually chemical in its nature, as in response to the demand for some sort of science an equipment for the teaching of chemistry was that first provided. This choice was to some extent unfortunate. Headmasters and assistant masters who had received only a classical, mathematical or literary training were ignorant, as many still are, both of chemistry and of its inherent educational value. As formulated then this subject did not touch daily life at many points, so that it was impossible to explain its virtues to men without previous training in any branch of science; consequently, they could not be brought to understand that it was necessary to make adequate provision, both as regards time and other school machinery, for its study; still more was this the case when ultimately it was necessary to provide for a sequence of studies in this and other branches of science.

The boys also were affected in somewhat the same way. For various reasons, chemistry had no social prestige amongst them, a contributory reason amongst others being that it was taken in the least valuable hours, under impossible conditions; consequently science was looked on rather as a "rag" than as a serious pursuit, the chief interest being the possibility of an explosion or of some experiment going wrong. To the majority of masters as well as of boys science figured as

"stinks" and the masters who taught it were regarded as inconvenient charlatans.

Another unfortunate effect of the universal adoption in schools of systematic lectures on chemistry as the main method of teaching science was that this course helped to shape and stereotype examinations in science along the lines which even nowadays are considered suitable by outside examiners for public school boys.

But in spite of the handicaps, the early science masters and their successors proved to be true pioneers. As conditions changed and opportunities offered, reforms were effected and progress has been made steadily till the present day; now an entirely different state of affairs prevails.

In the last few years, the larger public schools have spent over £200,000 in building and equipping laboratories. Improved facilities are offered for the study not only of chemistry but also of the other physical sciences and in many cases even of biology or at the least of some form of biological nature study. Most important of all, the ideal kept in view has been that the boy should have the opportunity of doing the work himself. The laboratory accommodation provided is such that experimental work done by the boys themselves is now the rule, this being supplemented by lectures to any extent that is desirable.

Simultaneously, in the case of senior boys, the drafting of whole forms *en bloc* to "science" has ceased. Only selected boys nowadays pursue the systematic study of science and these boys come in divisions, so that they can be graded according to their ability and attainments in the different branches of the subject. A sequence of studies is possible and many boys are granted adequate time for the study of science in its elementary stages.

Hand in hand with this movement the social position of the subject has changed. Formerly the boy who had an uncanny knowledge of the salts of silver or the compounds of chromium was looked on as a freak in his House and as a nuisance at home; even nowadays perhaps it is not the scientific training nor the scientific attitude of mind, if it have been acquired, which are appreciated but a high value is certainly attached to the scientific knowledge and information acquired incidentally to these. In his House, the boy possessing this knowledge is likely to be able to give interesting information about motor cars and aeroplanes or even hints of economic value as to

tapping the high pressure electric supply for surreptitiously charging accumulators and kindred purposes. In the same way, father and boy find they have something in common in discussing such matters, whereas paternal interest in Avogadro's hypothesis is limited. Meanwhile, our younger colleagues have themselves had some experience of a science teaching more educative than that at the disposal of their predecessors, whilst the seniors find parental pressure freely brought to bear that boys may have the opportunity of studying the kind of subjects to which I have referred and they also find that they meet schoolmasters of scientific attainments at dinner with comparatively respectable mutual acquaintances. If not less unsympathetic towards our efforts, they are at all events more tolerant of them. In another decade a generation of headmasters will be in office who were at school when the pursuit of science was no longer despised and even the captain of the cricket eleven was a science specialist without loss of prestige; altogether the future for training in the systematic study of science at public schools looks promising.

But this very improvement in the conditions affecting the study of science has brought other troubles in its train.

Consideration of plans for the improvement of special points in connexion with the teaching of the boys who may be regarded more or less as specialists has to some extent excluded reflection upon the more general aspect of our work. Following in the lines of classicists and mathematicians, we have in the past not avoided the danger of evolving schemes as specialists which, though admirable for specialists, were not necessarily the right ones for the average boy. Too frequently the sequence of studies has been determined by considerations of their logical order with a complete disregard for the suitability of the subject matter for the particular boys concerned.

Experience shows that the order of the boy's mental growth and of his developing interests does not necessarily coincide either with the logical order of the sciences or—though this is suggested more diffidently—with the historical order of the revelation and appreciation of scientific truths in the world's history.

Thus we find Prof. Porter in "Broad Lines in Science Teaching" making a direct attack on school science, chiefly on the conditions—which he describes as "meagre and restricted"

—but also on the teaching—which he suggests is "pure cram." But the logical deduction from the argument he uses is that the study of all science should be postponed till mechanics have been thoroughly studied under the wide and generous conditions of University teaching.

Now this would limit any study of science to those whose profession would lead them eventually to college classes and an intensive study of science.

It is a repetition of a now familiar plea. Every specialist wants his own subject taught from the beginning as though all the students were also to be specialists. Modern geographers recommend methods admirable for future specialists in geography and would force them on all boys alike; mathematicians argue as though all students were to be pure mathematicians; scientists are now following a similar course, though some of them are consistent and maintain that the whole civilised world must ultimately consist of trained, perhaps heuristically trained, scientists. But the average boy is not going to be a specialist in either geography or pure science or mathematics. The world, some would say unfortunately, needs a certain number of pure mathematicians to establish formulæ which other men will apply successfully. But these latter would never have discovered the formulæ themselves and their ability to use them is delayed and not helped by the attempt to train them to discover the formulæ. The expert work of one man is the raw material of another and surely the division of labour implied by this fact is the foundation of any progress whether in social or educational systems.

To Prof. Porter's contention that good teaching of mechanics should precede the study of physics many will agree when considering boys who *are* to have the opportunity of studying physics. But if one of the consequences be that, in order to attain this ideal for a certain few, all other boys must be deprived of the opportunity of any work at scientific subjects, there are many who will disagree. For we have to deal with practical problems under present conditions and not with ideals.

The nature of the problem is something like this. The specialists and those who pursue the systematic study of the subject are receiving due attention. Further reform for them depends now chiefly on the nature of examinational requirements. So long as the Universities elect to Science Scholarships in Chemistry boys who do not know anything about such daily

life chemistry, for example, as to whether they live on a clayey or on a sandy soil, if on either; as to fermentation or what the object may be of preventing the bacterial invasion of an egg or leg of mutton; or anything about the problems of oxygenation in so far as these affect their own health or the keeping of a fish alive in a tank, so long will juvenile experts on ionisation and electrons remain ignorant of these things. But there remains that large majority of boys who will never study science systematically. In most schools boys in some portion of the Lower or Middle School devote compulsorily certain hours to science. Consequently every boy is given the opportunity of revealing his inaptitude or discovering his aptitude as a student of science. The specialists who will pursue the further study of the subjects are recruited from these divisions, whilst for the rest this constitutes their one educational opportunity of receiving any scientific training or information.

Equally whether he be intellectually just an average boy or potentially a prime minister, archbishop or headmaster, the average member of these divisions will never get another opportunity of performing experiments in a laboratory, though if his interest ever become aroused he may achieve more knowledge by his own independent reading. Any benefit science can confer on him educationally has to be won at the best in some three hours per week during ten weeks as a minimum or sixty weeks as a probable maximum, *i.e.* in 30 to 180 hours of his lifetime.

There is a marked tendency to use these divisions at present simply as a training ground for those boys who later on will pursue the systematic study of science. Thus in physics much time is devoted to accurate weighing and certain other measurements as the foundation of physics. In chemistry these divisions are kept either at weighing or at experiments devised to give training in some kind of chemical manipulation or elementary methods—things which are of little use as knowledge in themselves but only as tools in a subject which most of the boys will never attempt. Board of Education experts not only commend but even recommend this practice of clearing the way for systematic work of a more advanced nature.

Thus the average boy never becomes aware that though physics involves the accurate measurement of quantities, it does

not consist of it alone, as he is completely excluded from any knowledge of physics by the conditions. Public school masters have justified the practice to themselves by a belief in formal training or discipline. In the prefaces of many elementary science text books and in the majority of articles urging the claims of science as an educational medium, one comes across the statement that its chief virtue is the excellent training it affords for various so-called faculties, such as the faculty of observation or of accuracy. Those who believe in faculty training maintain that not only are the powers of observation or of accuracy developed in connexion with special chemical or physical phenomena by a training in the methods of these sciences but also that there will result a sort of transferable over-flow, that the powers of observation and habits of accuracy in general will benefit from this formal training. In the same way it is urged that mental gymnastics in classics or classical repetition for a boy without any literary or linguistic aptitude may not make him a classic but at all events they do some good in that they will increase his reasoning or memorising faculty in general. It is argued, in fact, that a faculty can be trained irrespective of the medium used, that an overflow will accrue from this training which will be available when the faculty is required for other purposes than the special one in which it has been formally trained. Thus we have believed and acted as though the mind were an aggregate of faculties—observation, reasoning, concentration, etc. and that a gain in any special power was necessarily a gain to the faculty as a whole. The general belief is that learning to do one thing will mysteriously make other things not identical with it better done. A surplus of mind can be accumulated and expended on other problems than the one in which it has been acquired. Thus the subject is less important than the mental discipline and the acquisition of accuracy in weighing or cork boring can be justified; though the boy never reaches the stage of learning any physics or chemistry, at the least he will have acquired increased accuracy in numbers, in keeping accounts, even in telling fishing stories. Thus we are justified in getting boys ready to study science, though they will never do so. But during the last few years the science of experimental psychology has been brought into being. As the result of their experiments, the psychologists tell us that formal training confers but little benefit outside

the subject used. Unless there is some identity they suggest that improvement in one special power means little or no improvement in its general applications. Identity, in this connexion, means the existence of elements common to both the subject in which the training is given and that to which the so-called faculty is to be transferred. Thus addition assists multiplication because multiplication consists largely of addition. But increased accuracy in weighing does not increase accuracy in any other mental or manual occupation which has nothing in common with weighing.

Whether or no the experiments of the psychologists justify their inferences is an open question. But their inferences in certain cases appear to coincide with normal experience and common sense. At all events they coincide with a growing belief that the subject really does matter and that if a boy is to excel in any power, he must get knowledge and training in the subject to which the power is to be applied, however little contingent benefit may accrue outside the subject in which he gets the knowledge.

This, however, implies the corollary that in the early stages knowledge should be as wide and varied as possible. The faculty of observation affords an admirable analogy. For increased powers of observation it is not the training of the eye, the ear, the hand to extreme sensitiveness but the working up of well organised knowledge within the mind itself which is necessary. Powers of minute observation in some given direction will only be obtained by cultivating special knowledge and so interest to correspond. Powers of general observation will coincide with the cultivation of wide interests. Interest and knowledge are then twin sisters. But for interest to arise there must be some aptitude or capacity. Hence science for the non-scientific is not likely to be more educationally beneficial than classics for the non-literary mind; except in so far as information about certain scientific facts is desirable for general culture.

To attempt to arouse interest in the average boy or to train his mind by dosing him with treatment which is suitable for boys who will pursue the study of science systematically later on seems particularly futile. If he derive no contingent benefit from this training, the waste of his educational birthright is criminal. Weighing, specific gravity determinations, bulb-blowing, cork-

boring, separation of sand and salt and a host of similar manipulations should disappear from the programme of the average boy either as ends in themselves or as objectionable preliminaries to be mastered by a select few for whose benefit the majority must be sacrificed.

In fact many of us think that it is this very consideration of the select few at the expense of the rest in other subjects than science which is responsible for the comparative intellectual failure of the average public school boy.

All through their school career the majority of boys are taught with reference to an ideal far beyond their capacity and methods suitable to this ideal are in vogue. The ideal, of course, is the production of such erudite classical stylists or embryo pure mathematicians as may win scholarships at the Universities.

The result is that work for the average boy, instead of depending on his reasoning power and stimulating his mental self-reliance, is reduced to mere memorising. Consequently there is a loss of plasticity and a lack of resourcefulness which are highly detrimental to him in earning his own living. Now that only a small percentage achieve administrative posts and the professions are overcrowded, an increasing number of average boys seek a livelihood as producers in the Empire either at home or Overseas. There is a useful place for every citizen of adequate moral strength Overseas and it is an Imperial obligation upon us to make them as useful there as possible.

But plasticity, resourcefulness and self-reliance are exactly the necessary attributes. Hence it becomes more and more important that science masters by their methods should seek to strengthen and not thwart these characteristics as far as possible. Especially since it is obvious that for the successful development of the resources of the Empire these boys need as far as we can give it to them a knowledge of the phenomena of their environment and the laws controlling them. Even if intellect can be trained, we must not forget that she is a female. Consequently perfection lies for the average boy not in the intellect so much as in her dressmaker; but there is a danger of our trying to deck her out in as ill-fitting garb as do some of our colleagues in other subjects.

Ideally it would probably be best for all boys of suitable capacity to pursue the study of science and possibly engineering.

They would then have been trained in accurate thinking, concise methods of investigation and clear expression of results With a wide and generous syllabus they would acquire some knowledge of their environment and the laws controlling it. If they did not lose their plasticity and intellectual self-reliance in the process, they would prove invaluable Imperial assets in various producing capacities, even though an engineering training only produced the above results for engineering and did not endow boys with them for all or any of their possible life occupations.

But meanwhile, owing to causes overwhelming at the present, most of the boys under consideration are not able to avail themselves even of the systematic science which does exist at public schools. Hence the importance of organising the divisions in which such boys get their one opportunity of a little science, so as to do them the maximum of good and the minimum of harm. There should be no difficulty in arranging a course that should also be profitable to boys who will begin the systematic study of science on leaving these divisions.

Under existing conditions the writer has found the following scheme, here briefly outlined, gives the most satisfactory results in stimulating interest and developing a frame of mind favourable to the independent acquisition of further knowledge later on and also in giving the boy a certain conception of scientific method which has stayed with him.

Experimental work bearing on whatever problem may be under investigation is done throughout by the boy himself and this is accompanied by occasional demonstration, information giving, summarising and heuristic lecturettes. The practical work is taken in small divisions which make a form of heuristic seminar possible. The boys begin with simple Natural History and the work of great biologists such as Darwin, Pasteur, Lister. The matter is not taken as a "subject" but simple chemical and physical phenomena necessary to the issue are considered; thus the presence of an atmosphere, the nature of oxidation, respiration and combustion are investigated experimentally as they occur. It is neither chemistry nor physics nor biology but all these "subjects" are drawn upon as necessary. The boys use simple microscopes as well as barometer tubes, air pumps and test-tubes in the same course;

they are growing moulds or infusions of *B. subtilis* one week and investigating the properties of oxygen the week after. The net result is some knowledge of a variety of subjects ranging from malaria and phagocytosis to vaccination and sterilisation, of a certain amount of hygiene and of some simple chemical and physical ideas.

The next course is rather more definitely chemical in its nature. Primarily it is based on the complete chemical investigation of some common substance, *e.g.* chalk, a candle, etc., by the boys themselves. The method is to proceed from the known to the unknown and it is during this course that a scientific attitude is more definitely fostered. But again physical, chemical and biological properties are taken together as they call for attention and not separated into water-tight compartments. Real experiments in connexion with such daily life experiences as breathing, burning and decay, soils, disease, etc., can be as truly educational as crystallisation and fractional distillation.

The boys then pass on to more definitely physical work, *e.g.* the electric installation of a house, using not toys but real commercial instruments—ammeters, voltmeters, motors, dynamos. They continue at this type of work till promoted beyond the reach of science or till they begin its formal pursuit, there being in addition to the above electrical course others on practical applications of the laws of heat and light, mechanics and sources of power, as well as other electrical courses including the making and use of telegraphs, telephones, electric bells, induction coils and wireless telegraphic apparatus.

In 120 hours a boy can get a good deal more scientific information and rather more scientific training than is possible in the like time for his examination-ridden brother of similar capacity. A boy so trained will probably be ploughed in most conventional science examinations for boys of his age. But the writer firmly believes that he would keep his mental plasticity and his interest in scientific subjects and respond to his environment more intelligently than do many average boys differently trained.

OVERDUE REFORMS IN THE TEACHING OF ELECTRICITY

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It is well to turn occasionally from the work of the pioneers of science and of those who adapt their discoveries to the improvement of the material condition of mankind to the consideration of problems that confront the men engaged in training the coming generation, as on their success depends the supply not only of future pioneers but also of a body of men intelligent enough to appreciate the value of such pioneers and to make profitable use of their discoveries.

There has been of late a growing tendency, in discussing educational problems, to consider the interests of the ordinary boy and less exclusively those of the future specialist. A considerable amount of pressure of public opinion has been needed to produce this attitude of mind, for it is not a natural one to the specialists who directly administer the several branches of education; they are inclined to treat an elementary course as necessarily a foundation for more advanced study of the subject and to regard those pupils, who through inaptitude do not pursue it beyond the early stage, as being comparatively unimportant. We are however now beginning to recognise that even boys who are destined to carry on their school studies up to a late age must specialise within a limited range and that it is important that their work should be profitable to them in those subjects which they will not continue to study after they are about sixteen years old. Hence it follows that in a secondary school the earlier course in every subject should be designed to be complete in itself and at the same time to provide so far as possible a suitable training for those comparatively few but important boys who will specialise in that subject.

This arrangement suits also the many boys who will not

pursue an academical career after leaving school; it is in the intellectual equipment of such boys for their future life that the public schools seem most open to criticism. A study of the subsequent careers of public school boys reveals many startling discordances between the estimates of their abilities formed by their teachers and by the world, which are too numerous to be accounted for on the plea of occasional late development. It may well be that the faulty methods of appraising ability at school are due not only to the imperfections of examinations but also to unsatisfactory treatment of the subjects of study—a consequence of the teaching being necessarily entrusted to experts who tend to work along the lines which they think best adapted to producing experts. There is much to be said for the view that what the world needs most is the genius and next the well-balanced mind; and that the mere specialist tends to become narrow and unfit to be trusted with great responsibilities. The true genius rises superior to mistakes in his early training, whilst the ordinary boy may be forced by wrong treatment into an attitude of antagonism towards intellectual effort which will handicap him throughout his life. To design machinery for discovering and training experts at the expense of the average boy may therefore be ill advised. Our public schools are admirable institutions for fostering talents and may not be destructive of genius; but it is only of late years that they have undertaken seriously to provide for the large number of boys who leave from forms below the sixth—perhaps because such boys drop out quietly and the great stimulus of endowments has never affected them.

But even if this view be a mistaken one and the primary object of schools be to produce specialists, it still remains true that it is of great importance to them to get the best out of their work in subjects which they drop early because they are not those in which they will specialise.

As an illustration, let us consider the course of Physics which is usually provided for public school boys. Arguments for teaching science to all schoolboys are not far to seek but does the science taught them always fully justify the arguments used? It is hardly an exaggeration to say that the ordinary grown man just at present regards science as synonymous with electricity—at any rate he would think first

of that subject in connection with science teaching; so we will see how electricity fares in the typical public school science course.

It is probably a by-product of the epoch-making work of Kelvin that many physical subjects were at one time regarded as somewhat beyond the capacity of young boys, on account of the considerable amount of mechanics which is required for their adequate treatment. It was very easy to design an elementary course in Heat, Light or Sound which makes no such demand; as it was less easy to do so with Electricity, it became the custom to take boys through successive courses in Heat, Light, Sound, Mechanics, Magnetism, Frictional Electricity and then Current Electricity. Before the last was reached, all the pupils except the science specialists had usually dropped out, ignorant even of the elements of a subject which probably interested them more than did all the others. In dealing with boys we are still open to the reproach of disregarding the processes natural to the immature mind, whether of the individual or the race; our methods of training are so highly developed that we are afraid to let a boy do anything at all until he has shown in an examination that he is trained for it. A child is encouraged to speak his mother-tongue even when his efforts are decidedly imperfect; his teachers at that stage are prepared to let him blunder but schoolmasters are too conscientious to suffer that and expect a finish and fulness of comprehension appropriate to a much later stage, thereby getting a very small output and killing all interest and freshness. We lead boys into a desert and keep them marching there, not even letting them climb Pisgah when it comes in their way; the great majority never reach the promised land. So "the man in the street," who was one of that majority, sometimes scoffs at our science teaching as too academic, whilst the boy resorts to his magazines and the motor papers, whence he gathers more of what he wants than he does from our lessons. In this way he gets rough but very illuminating ideas of telephones, wireless telegraphy, dynamos, the stability of aeroplanes, two and four stroke cycle engines and such like matters, teachers treating him the while as if he were a boy of the early Victorian period. There is much to be said for following the historical course of discovery in many subjects but the developments of applied electricity have so diverted the main road of progress

from Faraday's original line that the theory does not apply in this subject, at present at any rate.

It may therefore be worth while to consider whether it be not possible to teach current electricity to young boys, say of fourteen or fifteen, whose mathematical equipment is small and whose acquaintance with the rest of Physics is small or altogether lacking.

The first question is whether it be possible to dispense with a knowledge of electrostatics, for the delay in beginning electricity is caused by the necessity of acquiring enough mechanics to treat this subject with sufficient fulness. Useful testimony can be had from those who have tried to do so. Here a personal reference may be permitted. As a Civil Servant, I am precluded from publishing, even in the most general terms, any results of experience gained in the Government establishments with which I have had the honour to be connected and what is said in this paper has reference only to other schools, where of course the conditions are in many respects different; but some fifteen years ago I made this experiment at a great public school and each year's experience there only strengthened my conviction that it was workable and advantageous.

By following the lines of Clerk Maxwell's treatise on electricity, it is possible to produce an excellent course for boys of eighteen or nineteen who have a fair knowledge of mathematics and elementary mechanics. An outline of the steps leading to Ohm's Law in such a course will be useful for comparison with what is proposed for much younger boys. First comes a somewhat abstract treatment of electrostatics, in which charge is regarded as the end of a tube of force and much ingenuity is required in order to bring the mathematical problems of Attractions within range of schoolboys. The flow of electricity is then considered; electromotive force is defined on an electromechanical and current on an electromagnetic basis; the ratio of the former to the latter in a conductor is named its Resistance and this ratio is proved experimentally to be independent of the current density and electric potential of the conductor but to depend on its material, form and temperature.

This is a perfectly logical treatment of potential and Ohm's Law; but it is entirely unsuitable for young boys and will be more intelligible to older boys if they have previously acquired

a working knowledge of the behaviour of electricity in motion and at rest. It has one obvious characteristic, that the greatest difficulties are met with at the beginning.

The alternative method designed for young boys will not be described at length, as it has been adopted in some schools during several years past and is dealt with in detail in text-books. But as criticisms have been made of the treatment of Potential and Ohm's Law, an outline of the presentation of this part of the subject may be given. No knowledge of electro-statics is assumed; the course begins with experiments on cells, galvanometers, etc., which give some general ideas of current, resistance and electromotive force. A "legal" ohm is defined and the definition may be extended to the effect that n ohm coils in series have a resistance of n ohms. Copies are made by substitution for the standard in a circuit containing a battery and galvanometer. Although it could easily be shown here that a copy accurate for one current is accurate for all but that it must be at a definite temperature, it seems better to defer these points till Ohm's Law is reached. Next, the legal (electrolytic) unit of current is defined and a moving-coil ammeter thereby graduated or calibrated; the mechanics required for the comprehension of this instrument is regarded by the unspoilt mind as mere common sense.

Next comes the definition of the legal unit of electromotive force, based on the standard cell; previous experiments with batteries have instilled a belief that potential differences are additive, so moving-coil voltmeters of high resistance are calibrated or graduated on this assumption; as the calibration curve is found to be a straight line, interpolation is obviously permissible between the positions which the pointer takes up for successive additions of a standard cell to the battery used in graduating the instrument. Thus the units of resistance, current and electromotive force are defined independently and in such a form as to be at once applicable to numerical results. By considering a battery sending a current through a series of resistances, the boy gets a conception of the potential differences between the ends of a conductor as that fraction of the electromotive force of the battery which is employed in driving the current through the conductor. The relation between current, potential difference and resistance is then determined by experiments on conductors carrying a current furnished by a

battery, using the ammeter and voltmeter which he has calibrated; thus Ohm's Law is rediscovered in the form now used by practical men. We then consider the whole circuit and define battery resistance in the same manner as the armature resistance of a dynamo. The way in which we can lead up to absolute C.G.S. units at a later stage of the learner's career will be indicated later on.

For this course no attempt is made to give from the start a complete conception of electromotive force, potential difference and the distinction between them. The mechanical foundation is not reached at this stage; the boy is not ready for it but he gets a very clear working idea of electromotive force and potential difference sufficient for his present needs. On the other hand, it is very doubtful whether the ordinary school course of frictional electricity provides this complete conception. Few young boys completely grasp the meaning of a line-integral of electric force or the work done on an imaginary unit charge brought up from infinity. The lack of names for electrostatic units and the impossibility of any simple calculations in the ordinary course of frictional electricity are serious obstacles to the drill needed to force such ideas into a boy. And even if he were to understand it thoroughly, is sufficient stress laid on the identity of frictional and voltaic electricity to make him confident that what he has learnt of electricity in one connexion he can use in the other? It is customary to show a condensing electroscope or a quadrant electrometer with this object but I suspect that we really trust rather to the fact that the same names are used throughout.

The other important criticism is that this treatment of Ohm's Law is not logical. Even if it were not rigorous, experience suggests that the teacher's conscience would be the only sufferer; for over-emphasis on logic at that age defeats itself, as teachers of mathematics have recently shown with reference to deductive geometry. So we might confess ourselves better satisfied if we provided a young boy with a vivid conception, based only on such logic as he is fit to understand, than if we pursued a course to which no exception can be taken but which leaves no impression on his mind. As he grows older he can appreciate a more rigorous demonstration, perhaps all the better if the pace was not forced when his mind was too immature for it; and he has not to unlearn the truth,

though he may realise that he was not thoroughly justified in his belief. But is the method really illogical? I fully admit that the logic of the two alternative methods of treatment is of an absolutely different kind and for young boys I have no hesitation in choosing between them. Science should be taught inductively to such boys; the method here proposed for teaching electricity starts with the study of familiar phenomena and gradually works back to the fundamental principles, mechanical, electrical, magnetic, etc.; the alternative method may be characterised as a semi-deductive process, starting with a few fundamental abstract principles and working down to familiar phenomena and everyday processes and instruments. And it is legitimate to remind critics that in an inductive treatment of science we are limited in precision of statement to the degree of accuracy of our observations. If then we provide voltmeters whose resistance is so high that the current through them is much smaller than our possible error in measuring the currents in the conductors, it can hardly be held to be unscientific to use them, in an avowedly inductive course, in place of electrostatic voltmeters which take no current beyond that caused by imperfect insulation. It is not our object in this course to establish a small body of abstractions from which by processes of mathematics and pure reasoning we may draw precise deductions only roughly verifiable by experience. The methods of mathematical physics must be taught at the appropriate stage but they are ill suited to the present one.

But many experienced teachers will probably assert that frictional electricity is an excellent subject for young boys, even if it be demonstrable that it is not necessary as an introduction to current electricity. This view is well expressed by an extract from Tyndall's preface to the Christmas Lectures on Frictional Electricity which he gave at the Royal Institution in 1875. "I had heard doubts expressed as to the value of science teaching in schools and I had heard objections urged on the score of the expensiveness of the apparatus. Both doubts and objections would, I considered, be most practically met by showing what could be done in the way of discipline and instruction by experimental lessons involving the use of apparatus so simple and inexpensive as to be within everybody's reach." No words are needed, for those who have read his little book, in praise of the skill with which he carried out his object. But in this opulent

age the outlay on apparatus is condoned if it lead to valuable knowledge. The discipline which Tyndall advocates is in itself as sound as ever but is the instruction what is wanted now? That question fundamentally affects the suitability of a subject as an instrument of mental discipline.

There is strong warrant for the theory which deprecates the use of what may be called "artificial discipline" and attaches considerable importance to the intrinsic value of the subject-matter which is selected to provide scope for mental gymnastics. All the other subjects of the physics curriculum deal with phenomena continually met with in civilised life, even in that of a boy: all are of interest to the inquiring mind of childhood. This interest makes the discipline less unattractive and leads the boy to make an effort for himself which is of greater value than the most logical course elaborated by the teacher. More important still, he gains some idea of the application of scientific method to matters with which he will be in contact all his life and with many of which he is already familiar. But the school course of frictional electricity is almost ludicrously apart from a boy's environment, present and future.

Nevertheless, some study of the phenomena of electricity at rest is desirable and by making a further change in the traditional procedure it can be provided satisfactorily. This change is the substitution of voltaic for frictional electricity in teaching electrostatics. In the first efforts towards telegraphy frictional electricity was employed and success was not attained until this was given up and batteries employed; equal importance is of course not claimed for this innovation but there is a certain parallelism in the shifting of outlook, which I believe to be inevitable in this connexion and to be long overdue on the part of teachers.

There is no doubt that such a substitution is practicable, that it reduces the difficulties of experimental manipulation for boy and master and that it enables us to make use of the knowledge which the boy already possesses instead of transporting him to a region of unfamiliar phenomena.

As such a method of treatment is not customary it may be worth while to set out in some little detail a short course designed for boys who have gained some knowledge of voltaic electricity, to be included in the preliminary course of current electricity in the same way as electro-magnetics, current induction

and similar branches of the subject are usually introduced. In this manner electrostatics enters into the general study of electricity on the same footing as these other branches, according to its intrinsic merits and loses the preferential position it has held hitherto.

The object of the course is to give some knowledge of electric charges, potential on a more mechanical basis, the capacity of condensers and induction. The source of electricity throughout is the live wire of direct current supply mains (or, if they are not available, of a battery of about seventy-five Leclanché cells), the other main being usually earthed; a resistance of the order of 10,000 ohms should be put in the live supply main, so that no harm is done by earthing it. We first demonstrate the mechanical attraction between a charged body and "earth" by means of two gold leaves hanging side by side as in an electroscope but insulated from one another and about 3 mm. apart; one is connected to earth and the other to the main. This illustrates the principle of the ordinary commercial type of electrostatic voltmeter which is now introduced and used henceforth; the Ayrton-Mather type is found most suitable but the construction of a Kelvin multicellular voltmeter is more open to inspection. In either case the principle of its action can easily be understood from a diagram or model. Charges are next shown to be transferable by a portable conductor from the source of supply to the terminal connected with the movable plate of this instrument (the other terminal being earthed as usual); the movement of the pointer shows an increase of attraction with increase of charge. If it be desired it can be shown that such a carrier will take no charge from the inner surface of a hollow body connected with the live main; and for rough quantitative purposes the charge on a particular carrier, after touching the main, may be treated as a unit charge.

If the voltmeter be directly connected to the mains and the electromotive force increased or diminished by additional cells, the attraction is seen to vary with the electromotive force of the charging battery. Before this stage the boy will have become accustomed to the idea that the electromotive force of the source of energy is distributed round the circuit and he uses the phrase "potential difference between points on a current-bearing conductor" to express this idea. We now take him a step further towards a mechanical conception of electric poten-

tial by showing that there is a mechanical force of attraction between bodies connected to points of a conductor which are at different potentials, as he understands the term. To demonstrate this, the voltmeter is connected to the mains and its terminals are shunted with a high resistance of known value, comparable with that already inserted in the live main; he can calculate by Ohm's Law the fraction of the electromotive force of the source of supply that is employed in maintaining the current in this shunt, so he sees that if a potential difference exist between the fixed and movable plates of the voltmeter there is an attraction between them and he can graduate the particular instrument in use to measure potential differences in volts. We do not yet go so far as to define potential in terms of work or to calculate the relation which the force of attraction between two bodies, of stated form and relative position, bears to their difference of potential. But the pupil can now realise that it is possible to give a mechanical definition of potential difference, instead of the one he has used hitherto and that this latter then becomes a derived unit, adopted for its practical convenience, whose value may be determined by an absolute electrometer.

We next demonstrate the varying capacities of bodies, by showing how successive charges raise their potential and by showing that a plate, first charged to the potential of the mains when near an earthed plate, then insulated and removed from the earthed plate, suffers a considerable rise in potential; this brings in the idea of a condenser. The student is already acquainted with coulombs and he now learns the meaning of a microfarad and is introduced to the use of condensers of a commercial type. By experimentally sharing a charge between a charged and uncharged condenser of stated capacities and by simple reasoning, he reaches the equation $Q = CV$. If a condenser of 50 or 60 microfarad capacity be charged from the mains through a megohm, the charging current continues to flow during several minutes and if a microammeter be inserted in each lead he can watch the changes in the charging current and discover that equal currents flow through the two leads at any instant; hence he sees that there are equal quantities of positive and negative electricity on the plates of a charged condenser.

We next use a condenser to show some of the phenomena of induction. If both plates are insulated and one be raised to a

positive potential, the other is found to rise to the same potential by induction, the charges on either plate being very small; if the second be now earthed, a large positive charge flows to the first plate and an equal negative charge is induced on the second, as was seen previously. If both are then insulated and the first earthed, the potential of the second will fall to the same numerical value as before but negative. So it is seen that the potential of a body depends on that of its neighbours as well as on its charge. The principle is then applied to produce a charge at high potential; an insulated plate is connected to the live main, a sheet of paraffined paper is laid on it and on that a second plate with an insulating handle. The second plate is earthed and then removed and the potential rises so much that a spark can be taken from it. The substitution for the first plate of a rubbed sheet of ebonite, forming the ordinary electrophorus, demonstrates the production of electricity on ebonite by friction; the Wimshurst machine can then be introduced, if it be considered desirable to use it to show some of the properties of electricity at high potential.

To return to broader views, the basis of these proposals is the belief that school conditions make it highly expedient to modify the development of the subject so that it may be suitable for ordinary boys of fourteen or fifteen and at the same time may form a satisfactory introductory course for boys who will pursue the study of science up to a much more mature age. This object can probably be effected in more ways than one but the modifications which are proposed have been shaped in the belief that young boys learn more science if they are set to use actual tools and instruments in solving real problems, best of all if they use them on real tasks. The exigencies of teaching large classes often make this ideal impracticable but we can aim either at approximating to it as closely as circumstances permit or at hurrying the boy as rapidly as possible away from what he feels is the real thing and translating him into the realms of abstraction. To be profitable, abstraction requires a considerable concrete foundation. Therefore, so far as possible, everyday commercial instruments and conditions have been chosen in place of the usual apparatus to be found in lecture rooms and instructional laboratories, which appears to be designed primarily to materialise the ideal diagrams of the text-book and secondarily for verifying the results and laws therein reached by deductive

processes. Such text-books and apparatus are of value to the older student but do not appeal to the immature boy. His mind works inductively and he is interested in and treats with respect instruments which he knows are made for use and not merely for getting him through examinations.

It is true that the suggested changes in the course lead to a more directly utilitarian result but they are recommended not on those grounds but in the firm belief that they are more educational. We are apt to think that our duty as teachers is to turn out a finished product but it is a higher aim to turn out men who will never cease to improve. We are more likely to achieve this aim if we refuse the easy alternative of creating a special world wherein to develop our logical and abstract courses and accept the more difficult task of employing the conditions of real life as the medium of education.

It is of course easy to press a theory of this kind too far and difficult to reduce it to practicable working details; it is hoped that the constructive proposals here put forward will not be found either extreme or unworkable.

THE LUMINOUS EFFICIENCY OF ILLUMINANTS

By J. S. DOW, B.Sc., A.C.G.I., A.M.I.E.E.

During the last decade great improvements have been effected in methods of artificial illumination. In electric lighting our ideas of efficiency have been revolutionised by the introduction of metallic filaments and flame-arc lamps. In coal-gas lighting an equally remarkable advance is to be recorded and other methods involving the use of acetylene, of petrol-air gas and of paraffin oil in conjunction with the incandescent mantle have also come to the front.

At the same time our knowledge of the scientific basis on which the production of light reposes has also been widened and the physical and physiological laws governing the efficiency of illuminants are now much better understood. Yet this increased knowledge should keep us humble, as it has led us to realise that even the most efficient appliance hitherto devised reproduces in the form of visible light but a small percentage of the energy given to it. In the present article the writer will summarise some of the more fascinating researches recently carried out on the subject of luminous efficiency.

It is desirable at the outset to formulate a provisional definition of the ideal source of illumination. The criterion is one that might be approached from several standpoints: from the scientific point of view, we might assume that a light source, in order to be suitable for everyday uses, should (1) develop all its energy in a visible form and (2) that this energy should be distributed throughout the visible spectrum in such a way that the colour of the light closely resembles that of normal daylight. The definition would probably not be considered satisfactory under all conditions: as we shall see later, circumstances may arise when other qualities are needed in an illuminant; but it will serve as a starting-point.

A great many researches have been carried out recently

with the object of ascertaining what percentage of the energy given to modern illuminants reappears as useful visible light. These researches have involved the exploration of the spectrum of illuminants with a bolometer or thermopile, the percentage of energy occurring in the luminous form being subsequently deduced graphically from the energy curve thus obtained. In some cases special absorbing solutions intended to separate the non-visible from the luminous energy have also been used. The results are not entirely harmonious, being affected by certain experimental difficulties and by the definition adopted for the "visible region" of the spectrum. A distinction (originally not sufficiently appreciated) has now been drawn between the percentage of radiated energy appearing in a visible form or "radiant efficiency" and "luminous efficiency," by which latter is meant the corresponding percentage of the total energy given to the lamp, including conduction and convection losses.

The one broad fact which stands out from the researches of different investigators is that the efficiency even of the most modern illuminants is remarkably low. We might perhaps select as typical the following series of figures derived from a table published by Dr. H. Lux, representing the luminous efficiency of some common artificial illuminants:

Source.	Luminous Efficiency. (Percentage of total energy radiated as visible light.)
Petroleum lamp	0·25 per cent.
Incandescent gas: vertical	0·46 „ „
„ „ inverted	0·51 „ „
Electric incandescent lamp: carbon filament	2·07 „ „
„ „ „ tantalum „	4·87 „ „
„ „ „ tungsten „	5·36 „ „
Arc lamp, D.C.: enclosed	1·16 „ „
„ „ „ open	5·6 „ „
„ „ yellow flame	13·2 „ „

It will be noticed that in the case of the great majority of artificial illuminants all but about 5 per cent. or less of the energy supplied is wasted. When to this prodigious waste is added the considerable loss in the process of conversion of coal into coal-gas or into electrical energy, it will be seen how small a fraction of the original energy is ultimately applied to its intended purpose.

Yet a glance at the table shows that appreciable progress has been made. Until recently such progress has been in a

direction in part suggested by the researches of Lummer and Pringsheim. Their investigations showed that the nature of the radiation emitted by a "black body" can be exactly predicted if the temperature of incandescence be known. It has been laid down by Kirchoff that a body is capable of emitting only the radiations which it absorbs: consequently a truly black body absorbs and therefore emits all varieties of radiation; when heated it produces a confused series of vibrations of differing frequency but, as the temperature of incandescence rises, the dominant frequency increases according to a well-defined law. When the temperature is low no vibration rapid enough to be visible is produced; as the temperature is raised, however, a red glow appears which changes to yellow and ultimately to white, as the colours at the blue end of the spectrum are accentuated.

Broadly speaking, the radiation from most incandescent solid illuminants, such as the electric glow-lamp and the flat flame gas-burner, resembles that of a black body. The chief defect of incandescent solids is that a vast amount of non-luminous energy is produced and the luminous efficiency is correspondingly low; their chief advantage is that, owing to the sequence of vibrations produced, we obtain a continuous spectrum: consequently the colours of objects seen by their light are, generally speaking, not unlike those seen by daylight.

Lummer and Pringsheim have shown that the maximum of the curve of the energy radiated by an incandescent black body, at low temperatures far out into the infra-red, is advanced towards the visible spectrum as the temperature increases. The percentage of energy within the luminous region is therefore remarkably increased and the visible light emitted has been found to be approximately proportional to the twelfth power of the temperature. It is calculated that at the enormous temperature of the sun the maximum of the energy curve would be located immediately over the mid-point in the visible spectrum—the ideal position from the standpoint of light production; Dr. Drysdale has calculated that under solar conditions as much as 50 per cent. of the energy of radiation would appear in the form of visible light.

The researches of Lummer and Pringsheim serve to show the importance of the tendency of modern improvements in illuminants: there has been a progressive increase in tempera-

ture and a corresponding gain in luminous efficiency; at the same time, the lights have become whiter in tint. Thus metallic-filament lamps yield a whiter light than the old carbon filament; if we consider the past, we find light sources of a still ruddier hue in the gas flame, the petroleum lamp and the pine-wood torch of the Middle Ages.

It will be observed that the light from all the sources mentioned is derived from incandescent solid particles and the nature of their radiation is too nearly akin to that of a black body to constitute them highly efficient illuminants. In Fig. 1 the writer has plotted the energy curves of the sun and of several artificial illuminants and also the well-known luminosity curve for the eye throughout the spectrum. The maximum sensitiveness of the eye is located, it will be observed,

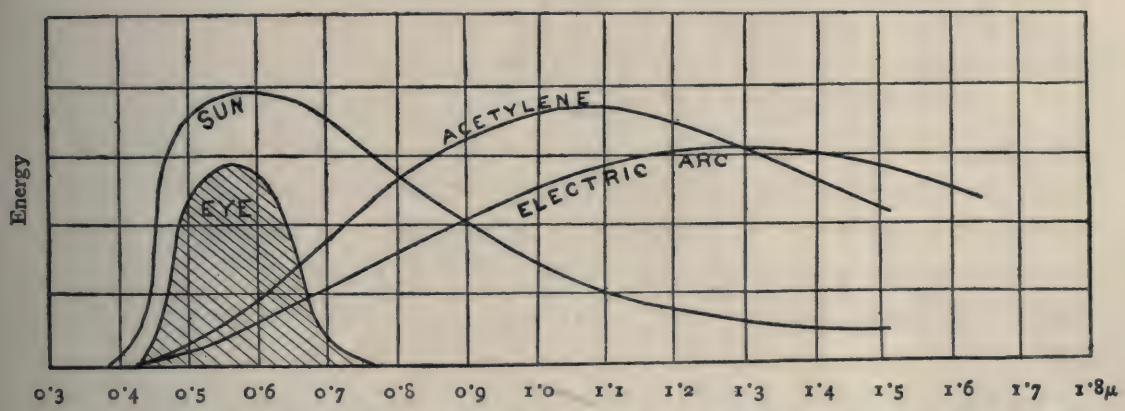


FIG. 1.—Showing distribution of energy in spectra of sunlight, acetylene and arc light, and sensitiveness of eye.

in the yellow-green at about 0·55μ. The maxima of the energy curves of the artificial illuminants fall far without the visible spectrum and the proportion of visible light produced is correspondingly minute. But it is most interesting to observe that the energy curve of the sun has its maximum very near that in the luminosity curve of the human eye. This suggests that our vision has been so developed as to make the best possible use of natural light. Presumably if the maximum of this energy curve had been located in the blue, our eyes would have so developed as to be most sensitive to light of this colour and our perception of colours in general would have been radically different.

In passing it may be pointed out that this raises one very interesting question, by which the validity of this theory might

perhaps be tested. It is well known that the quality of sunlight is considerably modified during its passage through the earth's atmosphere, which absorbs the rays of short wave-length to a greater extent than those at the red end of the spectrum. It might therefore be supposed (and this has been verified at the summit of Mount Whitney in the Sierra Nevadas by Prof. S. P. Langley) that the light in mountainous districts would contain an abnormally strong blue element and that the eyes of inhabitants of such regions would prove on examination to be specially sensitive to this part of the spectrum. In the like manner we should expect to find that the vision of habitual town-dwellers, accustomed to smoky and misty conditions, would differ appreciably from that of country people living under a clearer sky.

At the present time it hardly seems probable that the temperature of incandescent illuminants could be increased sufficiently to bring their energy maximum anywhere near to the visible region of the spectrum. There is, however, room for progress in this direction, seeing that present limits are set by other purely practical considerations: for example, it seems at present very difficult to make trustworthy metallic-filament lamps, having a reasonably long life, with a specific consumption much below 1·2 watts per candle; yet the consumption may easily be reduced to 0·5 watts per candle or less by overrunning the lamp and even maintained at this limit during a short time. The higher efficiency is thus an obvious possibility; the difficulty is that the filament cannot long withstand such conditions.

There is, however, another possible means of improving the efficiency of illuminants of which good use has already been made, namely, to choose materials departing from the black body law which exhibit "selective radiation." Substances which are highly polished, white, grey or coloured, may be expected to exhibit this quality. What is needed for the purpose in view is a material which absorbs and therefore emits visible light as completely as possible but which acts as a highly reflecting substance towards infra-red rays.

It is supposed that the Nernst filament and the incandescent mantle manifest selective radiation of this kind. It would take us too far afield, however, if we were to enter into the many interesting speculations that have been made as to the action

of the mantle; but it appears to be well recognised that its high luminous efficiency is partly due to selective radiation.

At the time of the introduction of metallic filament, a lively controversy took place regarding the explanation of the higher efficiency of the new lamps. By some authorities, the improvement was ascribed simply to a higher temperature of incandescence; others contended that the temperature was actually *lower* than that of the carbon filament and that the gain in efficiency must therefore be due to selective radiation. At the present time, the temperatures attained under practical conditions by the various metallic filaments appear to have been computed fairly definitely and it seems to be agreed that this is considerably above that of the carbon filament; but the recent work of Hyde, Coblentz and other workers in the United States makes it probable that selective radiation is also partly responsible for the improvement in efficiency.

Coblentz has recently carried out a most interesting series of researches on the radiation from polished specimens of various metals. His conclusion appears to be that metals which yield highly efficient lamp filaments also exhibit high reflective power and therefore marked suppression of radiation in the infra-red. Thus the reflective power of tungsten was found to be 50 per cent. in the yellow of the visible spectrum, 89 per cent. at $2{\cdot}5\mu$ and as much as 95 per cent. at 10μ. This means that there is a diminution in the amount of useless infra-red radiation and that the luminous efficiency is correspondingly improved. We do not know yet how far it is possible in practice to improve the luminous efficiency of solids by this means. But in view of the enormous extent of the infra-red range of radiation from most incandescent materials, the results of even a small suppression in this region might be very beneficial.

Another curious fact mentioned by Coblentz is that the quality of low emission in the infra-red is associated with good electrical conductivity. Metals having a high conductivity possess this quality in a high degree and therefore make efficient sources of light when their other physical qualities are such that they can be used. Most oxides, on the other hand, which are poor conductors, also possess a uniformly high reflecting power in the infra-red. It would almost appear as though the high electrical conductivity which proves so em-

barrassing in the manufacture of metallic-filament lamps is a sacrifice which we have to pay for their abnormally high luminous efficiency.

It seems probable that it would be difficult to secure anything approaching the theoretically ideal luminous efficiency by means of an incandescent solid. This, at least, is what our present experience suggests. Yet, as mentioned previously, the fact that solids afford a continuous spectrum is a considerable advantage from a practical standpoint, as the light allows reasonably good colour definition. It is even possible, by means of a special screen, to convert the light from a tungsten lamp into a close approximation to daylight. This appears to have been effected recently in the United States by Dr. H. E. Ives—naturally at the expense of a certain sacrifice in efficiency. Lamps provided with suitable screens to give the daylight effect are stated to have a specific consumption of about 10 watts per candle; the high consumption of power, however, is often of small consequence in comparison with the advantage of gaining an "artificial daylight" enabling delicate colour-matching work to proceed by night. Similar screens have also been devised for use with enclosed arc lamps and Mr. T. E. Ritchie, in a recent paper before the Illuminating Engineering Society, has pointed out that the resemblance of the light from an inverted open arc lamp to daylight is very much closer than is generally supposed—indeed, he thinks, probably closer than that of any other illuminant commonly used in this country. At this meeting a tungsten lamp provided with a special daylight screen was also shown for the first time by Dr. Kenneth Mees and it was stated that the resemblance to natural daylight is exceedingly close.

Assuming that we can specify what we mean by white light sufficiently closely for practical purposes, meaning thereby the colour of normal diffused daylight, it is of interest to ask what the ideal efficiency would be of a source of light producing these particular rays in the desired proportions and no others. The "mechanical equivalent" of white light has been studied by a number of observers and the results have been subjected frequently to revision. It is not possible, of course, to deduce a factor for converting energy into light in the same rigid manner as can be done for heat energy. We can, however, estimate what would be the output in candles per watt of our ideal

source producing only visible white light and no extraneous non-luminous vibrations. According to the latest determination of P. G. Nutting, who has studied this matter with special care at the Bureau of Standards in Washington, U.S.A., such a source would yield 26 C.P. per watt. Seeing that the most efficient artificial illuminant in use to-day does not produce more than about 4–5 C.P. per watt, even when considerable latitude is allowed as regards colour, it is evident that we are still a long way from the desired goal.

In what has been said above, it has been assumed that our ideal illuminant should not only emit all the visible vibrations in the spectrum, but should emit these in the proportions in which they constitute daylight. In a sense, a source which achieved this might be said to have a radiant efficiency of 100 per cent., seeing that all this output would be within the visible spectrum limits. Yet it is clear that by disregarding colour and aiming only at the production of as bright a light as possible, *i.e.* the generation of a maximum amount of light for a given consumption of energy, considerably more efficient results might be obtained. It is obviously uneconomical to include the rays at the extreme ends of the spectrum, the deep red and violet, which are just on the borderland of visibility. Clearly if quantity of light were the only consideration, our right course would be to ascertain the precise ray for which the sensitiveness of the eye was a maximum and then produce only this variety of radiation.

The question arises therefore where in the spectrum this position occurs. There has been much misunderstanding on this point, partly on account of the difficulties involved in heterochromatic photometry and also on account of the complexities introduced by the physiological peculiarities of the human eye. In order to locate this point of maximum sensitiveness, it is necessary to obtain the curve of luminosity for an illuminant and also the curve representing the distribution of energy; then to divide the ordinates of the former by the latter. In this way a curve of luminous efficiency regarded as a function of the eye, which would be independent of the illuminant studied, would be obtained.

Now the difficulty that arises in this connexion is that the eye itself is not constant in this respect. At a high illumination its behaviour is profoundly different from that characteristic of

vision by very faint light. As Sir William Abney and many other investigators have shown, the maximum (sunlight) luminosity for high illuminations commonly occurs near 0·58μ; at low illuminations it is nearer 0·53μ; whilst with a fully dark-adapted eye and a very faint illumination indeed, it may even be found near 0·51μ.

There are other marked differences in the behaviour of the eye at high and low illuminations which serve to suggest that vision is accomplished in a radically distinct manner under the two conditions. According to one theory, vision at very low illuminations is accomplished by the aid of minute retinal organs known as the "rods" (which are presumed to be colour-blind), whilst at high illuminations an entirely different set of organs, the "cones," which are invested with the power of perceiving

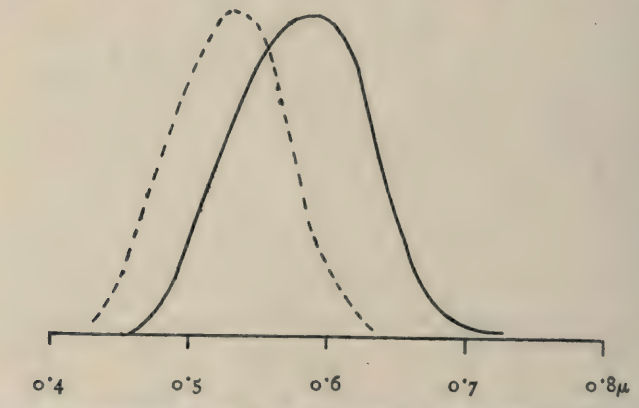


FIG. 2.—Showing sensitiveness of eye at high illuminations (curve drawn full) and at low illuminations (curve drawn dotted).

colour, come into play. The rods are also credited with being practically blind to red light but very sensitive to blue-green light, which, however, is perceived as white. Consequently in the faint twilight red objects, such as geraniums, appear dead black, whilst green grass appears an uncanny grey tint.

It would carry us too far away from the subject of this article to pursue the discussion of these fascinating peculiarities of the eye, some of which have been proved to have a very direct and even disconcertingly direct bearing on photometry. The phenomena are well established but the explanation seems to be still the subject of dispute among physiologists. For example, Dr. F. W. Edridge Green has expressed disagreement with the view of the functions of the rods given above, preferring a theory of vision based largely on the behaviour of

the visual purple. Whatever the true explanation may be, however, it seems clear that the quality of light which gives the best results with fairly high illuminations of say 1–10 foot-candles, such as occur in everyday practice, may be very different from that which it is desirable to use when very faint lights are concerned. For example, yellow monochromatic light might prove the most efficient for brilliantly illuminating an advertisement placard. But for marine signals, the value of which is mainly judged by the limiting distance at which the light can just be seen as a luminous point, greenish-blue light might be preferable.

The great majority of industrial uses of light, however, demand a fairly high illumination and should therefore be judged on what may be termed the upper register of vision. Acting on this assumption, P. G. Nutting has recently calculated that the most efficient light for creating brightness is that of wave-length 0·54μ. If it were possible to secure a source which produced this quality of light *and this alone*, he estimates that theoretically an efficiency of as much as 65 C.P. per watt should be possible.

Needless to say, we have no commercial sources of light at present which even approach this degree of efficiency. As explained above, even the most powerful flame-arc lamps, in which correctness of colour has been sacrificed to some extent to brightness, probably do not yield more than 4–5 C.P. per watt; were a corresponding estimate of the luminous efficiency of high-pressure gas lamps to be made, it is probable that the luminous efficiency would not reach this value. Indeed the comparatively low luminous efficiency of incandescent gas lamps is a somewhat hopeful augury for the future. It leads us to believe that the practical limit of possible improvement by using the waste heat of combustion more scientifically is still far distant. Seeing, therefore, that these lamps are already among the cheapest in existence as producers of light, it seems conceivable that in the future they might enable us to generate light on a scale far greater than that realised at present.

Incandescent gas lamps, it may be mentioned, yield a continuous spectrum, so that the further additional means of securing improved luminous efficiency—specialisation in yellow-green light—has yet to be applied. This principle has, however, already been used to some extent in artificial illuminants.

For example, the latest forms of yellow flame-arcs owe their efficiency largely to the fact that the carbons are impregnated with materials giving several vivid lines in the yellow-green. These lines are superimposed upon the continuous spectrum of incandescent carbon and naturally distort to some extent the colour of objects illuminated by them.

It is to be expected that line spectra judiciously used may lead to efficiencies much in excess of those obtained by the use of incandescent solid materials, which are thrown into a mere jumble of vibrations, some of which are valuable for the purpose in view but the great majority non-luminous.

But when "luminescence" is set up by the electrical stimulation of rarefied gases or metallic vapours in a vacuum tube, free natural vibrations come into play and if these are such as to produce the sensation of light the luminous efficiency is correspondingly high.

As explained above, most artificial illuminants which utilise luminescence also depend for their light upon incandescence to some extent and we therefore obtain a line-spectrum superimposed upon a continuous spectrum. Possibly this also takes place to some extent in the case of the Moore vapour tube and the quartz tube mercury vapour lamp, the spectra of which exhibit a considerable number of lines. We may naturally suppose that the spectra obtained from such vapours would depend on the pressure within the tube and that the dominant colour would be determined by the nature of the gas. Nitrogen yields a pinkish light, neon a bright reddish-orange, whilst carbon dioxide is said to afford a white light so closely resembling daylight as to have proved very suitable for silk mills and in other cases in which exact colour-matching is essential.

Perhaps the best-known example of a source of light depending upon luminescence is furnished by the mercury vapour lamp, which yields four groups of bright lines in the visible spectrum, situated respectively in the yellow, green, blue and violet. In addition there are also a considerable number of lines in the ultra-violet. It has, in fact, been estimated that whereas only a fraction of 1 per cent. of the total energy from the electric incandescent lamp takes the form of ultra-violet rays, as much as one-third of the energy produced in the spectrum of luminescent mercury is in the invisible ultra-violet

region. In the case of the ordinary mercury vapour lamp, these rays are nearly all absorbed by the glass tube and converted into heat; but more recently lamps have been constructed with tubes of quartz glass, which allows the ultra-violet energy to pass unchecked. This invests such lamps with remarkable properties to which reference will be made shortly. It appears that very little infra-red energy is produced in the mercury spectrum.

The utilisation of luminescence in the mercury lamp is the explanation of its high luminous efficiency. On the other hand, it is also a striking example of the imperfect control we at present possess over this system of producing light, as the peculiar greenish colour it exhibits unfits the lamp for many practical purposes. The experiences of those who have attempted to modify the spectrum of the mercury lamp are of some interest. Until recently all attempts to supply the missing red element in the light had proved fruitless. One of the first methods tried was to introduce gases into the tube, the idea being that a combined spectrum containing all the primary colours might be obtained by this means. Unfortunately it proved to be difficult to produce both the spectrum of the gas and the mercury vapour simultaneously. For example, when nitrogen was used the colour of the light was either pink or green, according to the temperature. Metallic ingredients, lithium, potassium, etc., were also tried but the results were again untrustworthy and not permanent.

It appeared therefore that a successful attempt to improve the colour must be made by some means outside the tube. One early device consisted in the combination of the mercury lamp with an outside source, such as a carbon-filament electric incandescent lamp, which is rich in red rays. This, however, proved an inconvenient complexity and naturally entailed a serious sacrifice in efficiency.

Within the last year, however, an announcement has been made by Dr. Cooper Hewitt of a method which appears much more promising. This consists in placing above the tube a reflector coated with rhodamine and other substances which fluoresce, transforming the blue rays partly into red radiation. As a result, the appearance of the light is much improved. It will be interesting to watch the future of this device.

This example of the practical utilisation of fluorescence for

illumination naturally leads us to consider the allied phenomenon of phosphorescence. By this term is generally understood the transformation of light from one frequency to another which persists after the exciting stimulus is withdrawn.

Inventors have long cherished the fascinating idea of a means of securing a cold phosphorescent light, portable and self-contained, which should absorb light by day and return it automatically when darkness had fallen. Fanciful such a suggestion may well appear at present, yet it might conceivably be realised in the future as a result of the persistent patient research on phosphorescence and fluorescence which is now going on in various parts of the world. At present, there are obvious difficulties. Phosphorescent materials, even under the most favourable circumstances, yield but a feeble light of short duration. Moreover the colour of the light yielded by most phosphorescent substances, generally a spectral blue or green, is hardly suitable for practical illumination.

Yet within recent years appreciable progress has been made. Our stock of phosphorescent materials has been considerably enlarged. We understand better how to prepare such substances in a highly active state and we have at our command a much greater range of colour. Moreover varieties of insects are known which give light of almost every conceivable colour and sometimes of a surprising intensity and the remarkable effect of phosphorescent organisms on some of the Canadian lakes has often been remarked. May we not, therefore, hope ultimately to produce a culture of bacteria gifted with special light-giving qualities? And may not chemists eventually succeed in producing synthetically and in large quantities the substances which yield the light in the case of these phosphorescent insects?

Another respect in which progress has been made is in the means of exciting phosphorescence. The new mercury lamps having tubes of special quartz or uviol glass are exceptionally powerful producers of ultra-violet rays, on the exciting action of which the phenomena of fluorescence and phosphorescence largely depend. By the aid of such lamps wonderful effects are said to have been produced in America, although little has yet been published on the subject. The writer was recently given to understand that its success for the production of novel stage effects has already been proved. Mechanical butterflies coated with luminous paint are excited and then set in motion and by

the same means the dress of a danseuse can be made to gleam with living fire. It has even been found possible to produce in the laboratory, by means of phosphorescent paints, landscapes glowing with approximately natural colours.

The possibilities of phosphorescence appeal to the lighting engineer for another reason. There is good reason to believe that a source of light based on this principle would be extremely efficient. Prof. S. P. Langley many years ago studied the light from the firefly and came to the conclusion that its luminous efficiency was of the order of 100 per cent., all the recognisable radiation being concentrated in the visible spectrum. Now the photometric study of these faint phosphorescent substances imposes special difficulties and the bolometric methods ordinarily applied are excluded by the small amount of energy available. One must therefore exercise some caution in drawing conclusions as to their apparent high luminous efficiency.

Two American scientists, Ives and Coblentz, have recently repeated Langley's work on the firefly by a new and ingenious photographic method and were led to similar conclusions. Dr. E. L. Nichols, at Cornell University, has also made researches on a large number of phosphorescent materials and he, too, has been struck by the fact that the energy of such substances seems to be almost invariably concentrated in the visible spectrum. The light, as mentioned above, was usually of a greenish or blue tint but it is possible that by the judicious combination of various materials an approximation to daylight might eventually be obtained.

Let us, however, now return to a point referred to earlier in this article, namely the production of light of certain predetermined colours. It has been pointed out that, if our object were only to secure, as economically as possible, a light of great brilliancy, we ought to aim at producing the highly luminous yellow-green rays. For many purposes, *e.g.* in the case of searchlights, the illumination of railway yards, docks, etc., such a light would doubtless answer all requirements and would be vastly more efficient than most of our present illuminants. In foggy conditions, however, when " penetrating power " is mainly desirable, it might be preferable to aim at producing light of an orange-red character, since rays of long wave-length appear to make their way best through minute atmospheric particles. On the other hand, if we wanted a source which should be visible

as a luminous point a long distance away, it might be better to aim at producing the green rays.

There are also cases in which it would apparently be beneficial to use monochromatic light. Indeed in certain conditions some of the rays produced by modern illuminants may be actually prejudicial to the purpose in view. One instance of this is afforded when light is used not to "create brightness" but to "reveal detail." It is well known that the eye is not achromatic, *i.e.* cannot bring to a focus rays from all parts of the spectrum simultaneously. Ordinarily, we may suppose, the eye accommodates itself to the intermediate region, whilst the red and violet appear somewhat "fuzzy" and therefore spoil the definition of the object. Recent experiments lead one to suppose that acuteness of vision might be substantially increased by using monochromatic light. It has also been suggested that red light gives the best definition in the case of distant illuminated objects whilst blue and green light are best for near vision. This at least is the writer's experience but it appears that it may not be universally true for all eyes.

Another special use for coloured light is in connection with decorative effects. At present our methods of producing coloured light are based mainly on absorption and are therefore woefully inefficient. In order to produce blue light, for example, we start with a white light and then absorb and lose all but the blue, which is probably but a fraction of 1 per cent. of the total energy generated. Thus in obtaining pure blue light by this means from an ordinary metallic-filament lamp, screened with blue glass, only about 1/5000th of the energy actually given to the lamp would be ultimately used.

If, however, we could discover a means of generating only light of the one colour we desired to use, such light might prove to possess unexpected and valuable qualities. At present very little is known about the effects of monochromatic light; chiefly because we have no ready means of producing it in reasonably large quantities.

As an illustration of the possibilities in this direction we may take the well-known therapeutic qualities of the Finsen lamp. Its curative effect is ascribed to the blue and violet rays which are filtered out from the light yielded by a very powerful arc lamp; but it is only by the concentration and waste of a very large amount of luminous energy that this effect becomes apparent.

The peculiar physiological and psychical effects of coloured light hardly fall within the scope of this article but may be mentioned briefly. There is a popular impression (supported it would seem by scientific researches) that the red end of the spectrum is exciting and stimulating whilst the green and blue rays have a sedative or even depressing effect. Thus the colour red, apart from its well-known irritant effect on animals, has been habitually associated with violent ideas such as war, revolution, etc.

According to some medical authorities patients subject to melancholia are braced and favourably influenced by sitting in a red room, whilst excitable maniacs are often soothed by confinement in green and blue surroundings. In some oriental countries permanent enfeeblement of the mental faculties of inconvenient political prisoners is said to have been deliberately produced by prolonged exposure to blue light. In a concentrated form blue light is also said to produce local anæsthesia of the skin.

All these are matters which are mainly of interest to the physiologist and seem to require more substantial confirmation. But from the physical side it may be pointed out that the existence or non-existence of such phenomena could doubtless be much more readily studied provided we were able to secure pure red and blue light in large quantities. Meantime it is of interest to mention, in passing, that somewhat analogous experiments on the effect of coloured light on plants were carried out some years ago by Camille Flammarion. He showed that red light tends to produce abnormally rapid growth while blue light retards development. Also one of the sensitive mimosa plants was rendered comatose and unresponsive when placed in blue light but showed quite unusual excitement under red rays. The effect of coloured light on plants is thus another subject to which the remarks made above might be applied. Steinmetz has attempted to generalise on its influence by suggesting that the comparatively slow visible red rays and heat rays, by producing resonance with complex groups of atoms, are favourable to growth and life, while the more rapid blue rays do not readily produce any such quickening effect. Ultra-violet rays (which in excess are known to be prejudicial to plant life), on the other hand, are assumed to produce resonance of individual detached atoms or electrons, breaking down the molecule and leading to death.

Allusion has already been made to the generation of ultra-

violet energy in the quartz tube and uviol mercury vapour lamps. This matter may be recurred to here because it aptly illustrates the important consequences following the discovery of a source unprecedentedly rich in some special form of energy.

It has already been explained that the amount of ultra-violet energy obtained from incandescent solids is in most cases but a fraction of 1 per cent. Ladenburg, however, has calculated that as much as 30 per cent. of the energy in the mercury spectrum is located in the ultra-violet.

It is therefore hardly surprising to find that these lamps possess some remarkable properties. The influence of ultra-violet light in exciting chemical action may now be realised on an unprecedented scale. Their application to the sterilisation of milk and water, for tanning leather and for therapeutic work, has already attracted attention.

But perhaps the most interesting instance of their unexpected properties is in connection with the fading of colours. The gradual deterioration of all but the most permanent colours when exposed to bright sunlight is of course familiar and has long been attributed to the ultra-violet rays. So well was this recognised that the manufacturers of carpets and dyeing materials in the north of Germany were in the habit of sending their materials periodically to the sunny south where their permanence could be tested by abundant sunlight. But the advent of the new lamps appears to have changed all this. Being far richer than normal sunlight in ultra-violet rays it enables manufacturers of coloured materials not only to test their fabrics in their own works independently of climatic conditions but to do so in a far shorter time than was previously required.

It may be mentioned that excess of ultra-violet light is known to have a very prejudicial effect on the eyes. Special precautions are therefore necessary when using these lamps and when they are applied for ordinary lighting purposes the internal quartz tube is covered by a substantial outer globe of thick ordinary glass by which the ultra-violet rays are absorbed.

Yet it should not be hastily concluded that ultra-violet rays are necessarily inimical to health (as has sometimes been done). The cleansing sanitary effects of sunlight have been expressed in an Italian proverb, " Where the sun does not enter the doctor comes " and it is now believed that this beneficial action is at least partly due to the valuable sterilising action of the ultra-

violet rays present. Careful experiments have established the destructive action of these rays on bacteria.

In conclusion we may summarise the contents of this article by pointing out that in practice there are many special problems in illumination, each demanding not only a certain intensity but a certain *quality* of light. We are still far from the ideal of controlling our illuminants so perfectly as to generate just the variety of light we require and no other for a specific problem. Yet progress in this direction is being made. We may even picture the day when, by some easy adjustment, we shall be able to alter the light from a source at will and cause it to give out successively rays from any desired part of the spectrum—to play upon the visible gamut of light with the same ease and certainty as upon the audible octave.

VERTEBRATE PALÆONTOLOGY IN 1911

By R. LYDEKKER, F.R.S.

Of the various memoirs and papers relating to vertebrate palæontology published during 1911 which have come under my notice, none contains any startling new discovery and the summary of the year's work consequently appears as a record of steady progress in all departments, rather than of epoch-making advances. Perhaps the most important items relate to the discoveries in Africa and to the work accomplished in connection with the primitive reptiles of the Permian epoch.

As in past years, I make no claim to give a complete summary of the work done during the period under review but refer only to the more important papers which have come under my own notice. As the Palæontographical Society's volume for 1911 did not appear till 1912 was well advanced, its contents have not been included in this review.

A feature of the year's work is the relatively large amount of literature dealing with remains of animals from the early historic and prehistoric epochs and their bearing on the origin of domesticated breeds. Prof. J. C. Ewart, for instance, has furnished a chapter on the animal remains to Mr. James Curle's volume entitled *A Roman Frontier Post and its People; the Fort of Newstead, in the Parish of Melrose.*[1] In this chapter the author reiterates his well-known views with regard to the existence of several fundamentally distinct types of horses to be met with in the superficial formations of the British Isles and the Continent and their relationships to modern breeds and races. He has also much to say with regard to cattle, more especially the so-called Celtic shorthorn; but perhaps the most interesting as being the most novel portion of the memoir relates to sheep. The latter, it appears, are represented at Newstead by two distinct types, one of which is akin to or identical with the so-called turbary sheep (*Ovis aries palustris*) of the Swiss pile-dwellings, which was a long-tailed breed, with short upright horns like

[1] Glasgow, 1911.

those of a she-goat, whilst the second comes equally close to Studer's sheep (*O. a. studeri*), typically from the pile-dwellings of Lake Bienne, which closely resembled the wild mouflon of Sardinia and Corsica. Both these breeds are stated by Prof. Ewart to be represented in strata believed to be of neolithic age in the Thames Valley and certain other parts of England.

Studer's sheep appears to be closely related to the small brown-woolled breed inhabiting the island of Soa or Soay, near St. Kilda; and the author concludes that both are the descendants of the wild mouflon, which, by the way, is miscalled *O. orientalis*, whereas its rightful name is *O. musimon*.

Much the same views are expressed by Dr. E. L. Trouessart, in an article on the prehistoric origin of our domesticated animals (*Biologica*, vol. i. pp. 296–306, Paris, 1911); this writer, however, considers that at least some of the domesticated breeds are derived from the wild Armenian *O. orientalis*. In connexion with this derivation of domesticated sheep from one or other of the above-mentioned wild species, it may be mentioned that if either really occupies the position of ancestor to the European breeds, it is clearly entitled to the name of *Ovis aries*, as typified by the domesticated sheep of Sweden.

As regards the rest of the aforesaid article by Dr. Trouessart, it must suffice to mention that the author is convinced that the Indian wolf (*Canis pallipes*) is the ancestral stock from which at least many of our domesticated dogs are descended.

On the other hand, Dr. Conrad Keller, in a memoir on the domesticated animals of the Mediterranean Islands and their bearing on the origin of those of Europe generally (*Neue Denks. Schweiz. Naturfor. Ges.* vol. xlvi. pt. 2, 1911), has been led to conclude that the prick-eared Ibiza greyhound of the Balearics which in ancient times inhabited Egypt, is descended from the cuberow or wild dog (*C. simensis*) of the mountains of Simien, in Central Abyssinia; this opinion being based on the remarkable similarity between the skulls of these animals, which are distinguished from those of other dogs by their excessive slenderness and length. It is true, indeed, that the skull of the Balearic breed is distinguished from that of the wild species by the greater elevation of the frontal region but this is a feature of all domesticated dogs as compared with their wild relatives.

Bones found during excavations on the site of the Roman city of Corstopitum, near Newcastle-on-Tyne, in 1910, form the subject of an article, by Messrs. A. Meek and R. H. A. Gray, published in *Archæologia Æliana*, ser. 3, vol. vii. According to the authors the bones and skulls of many of the oxen agree closely with those of the white cattle of Chillingham and other British parks; a peculiarity said to characterise both being the absence or early shedding of the antepenultimate lower premolar tooth. On this ground Chillingham and Roman cattle are declared to represent a new wild species, for which the name *Bos sylvestris* is proposed; the fact being ignored that park-cattle already possess a scientific name—*Urus scoticus* of Hamilton Smith—and likewise that the colour of these cattle is decisive as to their domesticated origin. The alleged absence of the anterior premolar is probably a feature due to domestication.

Yet another paper dealing with Post-Tertiary formations is one by Dr. J. A. Grieg, published in the *Bergens Museum Aarbok* (1911, No. 5), on the prehistoric fauna of Hardangervidden, Norway.

Very little has been known hitherto with regard to the Post-Tertiary fauna of Portugal and it is therefore satisfactory to have a summary of the present state of our information on the subject by such a well-known specialist as Mr. E. Harlé, whose communication is published in *Comm. Serv. Geol. Portugal* (vol. v. pp. 22–86). For the most part, at any rate, the birds and mammals of these formations are the same as those from the corresponding horizons of other parts of southern Europe. Special interest attaches to the author's investigations with regard to the alleged occurrence of mummified lemmings in a Portuguese cavern. On the face of it, such alleged occurrence was in the highest degree improbable and Mr. Harlé considers that it is based on error, no traces of these rodents having been observed after a careful examination of cave débris.

In an article on mammalian remains in the museum of Troïzhossawsk-Kiakhta, Transbaikalia, published by the Société Impériale Russe de Géographie, section du Pays d'Amour, vol. xiii. pt. 1, Madame Pavlow records a number of well-known Pleistocene and existing species, together with extinct races of a few, such as the argali sheep (*Ovis ammon*) and the markhor goat (*Capra falconeri*) which have not hitherto been known in the fossil state.

In a communication to the *Centralblatt für Mineralogie*, Stuttgart, for 1911 (pp. 54–60 and 83–9), Mr. H. Stremme describes certain mammalian remains from the *Pithecanthropus* beds of Trinil, Java; referring those of a dog and of a cat to new generic types.

Turning to papers dealing with older faunas, reference may first be made to one by Dr. Franz Toula (*Abh. Geol. Reichsanstalt, Wien*, vol. xx. pt. 5, pp. 36–45) on mammalian remains from the Pliocene lignite formation of Illyefalva. Since, however, all the specimens—of which many are figured—pertain to well-known types, detailed notice is unnecessary. The same is the case with an article by Dr. Max Schlosser on vertebrate remains from the brown coal formation of Bruxer, published in *Sitz.-Ber. Ges. Lotos*, vol. lviii. pp. 229–45.

Mammalian remains from the Sebastopol district described by Mr. Borisĭak (*Bull. Ac. Sci. St. Pétersbourg* for 1911, pp. 241–50) indicate a fauna allied to that of Pikermi, including examples of *Aceratherium*, *Hipparion*, *Giraffa* and *Tragoceros*.

Of more general interest is an article by Dr. H. Stehlin on the evolution and dental development of ungulates from the Lower Tertiary *Bohnerz* of Switzerland (*Verh. Schweiz. Naturf. Ges.* 1910, pp. 138–68), in which it is recorded that as we pass from lower to higher beds there is an increase in the bodily size of the species, accompanied by an increase in the height and complexity of the crowns of the cheek-teeth. This progressive complexity is shown in the conversion of triangular, three-cusped premolars into square, four-cusped, molar-like teeth. Towards the close of the paper it is stated that the evidence of Tertiary mammals indicates free communication between the Old World and North America during the early Eocene, after which there was a sundering of the two continents, while union was re-established in the Oligocene. Africa during the Eocene seems to have had no direct communication with Europe, the relationship between the European early Tertiary lemuroids and the modern lemurs of Africa being capable of explanation by means of a land-connection by way of Asia.

In 1910 Dr. Stehlin also published the sixth part of his account of the mammals of the Swiss Eocene (*Abh. Schweiz. pal. Ges.* vol. xxxvi. pp. 839–1164), in which he described seven new species of artiodactyles.

In my review for last year reference was made to a preliminary communication by Dr. Schlosser on vertebrate remains from the Lower Tertiary of the Fayum district of Egypt. A fuller digest of the material on which the former communication is based appears in *Beitrage f. Pal. u. Geol. Öster.-Ungar. u. Orients*, vol. xxiv. pp. 51–161, in which the new species are more fully described, with, in some instances, a revision of their systematic position. The genus *Parapithecus*, for example which was at first included among the man-like apes (*Simiidæ*), is now referred to a distinct family (*Parapithecidæ*), characterised by a dentition differing from that of both anthropoids and lemuroids, the formula being *i.* $\frac{1}{1}$, *c.* $\frac{1}{1}$, *p.* $\frac{3}{3}$, *m.* $\frac{3}{3}$. The genus *Moeripithecus* (which is evidently allied to Osborn's *Aphidium*) may belong to the same family, although, on the other hand, it may be related to the North American Eocene genus *Anaptomorphus* and thus form an ancestral type of the American monkeys but if so, its dental formula, instead of the above, must have been *i.* $\frac{2}{2}$, *c.* $\frac{1}{1}$, *p.* $\frac{2}{2}$, *m.* $\frac{3}{3}$. The genus *Metoldobotes* (or *Metolbodotes*), originally included in the Primates, is transferred to the Insectivora, in which it occupies an undefined position. Special interest attaches to the occurrence in such early strata of remains of a large bat, for which the name *Provampyrops* is proposed.

In North America Mr. J. C. Merriam has published a paper (*Bull. Dep. Geol. Calif. University*, vol. vi. pp. 403–12) on the Tertiary mammal-beds of Virgin Valley and Thousand Creek, North-Western Nevada. Several new species are described; but the chief interest of the paper is the reference of certain supposed Tertiary antelopes with twisted horns to the same family (*Antilocapridæ*) as the American prongbuck. From the shape of their horns these ruminants (*Ilingoceros* and *Sphenophallus*) were at first supposed to be related to the bushbuck group (*Tragelaphus*, etc.) of Africa; but on distributional grounds, this was very improbable, while their relationship to the prongbuck is just what ought to occur. Both extinct genera are believed to be derivatives from the earlier *Merycodus*.

There seems but little to record in regard to the palæontology of rodents. Miss L. Kellogg (*Bull. Dep. Geol. Calif. University*, vol. vi. pp. 401–2) has, however, described a new beaver from the Tertiary of the Kettleman Hills, California,

on the somewhat slender evidence of a single incisor tooth. The grounds for specifically distinguishing this specimen are, firstly, that it occurs in a horizon from which remains of beavers have not previously been obtained; secondly, that the relative proportions of its two transverse diameters are the reverse of those which obtain in the corresponding teeth of other fossil American beavers.

Passing from faunistic papers to those dealing with individual groups and species of mammals, it may be noted that many years ago the name of *Arctotherium bonariense* was proposed for a huge bear-like carnivore from the Pampean formation of Argentina. Although remains of other species were subsequently discovered in the Californian and Pennsylvanian Pleistocene, the genus, which appears to be in some degree intermediate between modern bears and the Pliocene Old World *Hyænarctus*, has been regarded as a southern type. This conclusion was, however, somewhat premature, for during 1911 Mr. L. M. Lambe has described (*Ottawa Naturalist*, vol. xxv. pp. 21–6) the skull of a species from the Pleistocene of the Yukon. *A. yukonense*, as the new form is called, thus serves to bring the genus into connection, by way of Bering Strait, with *Hyænarctus*, and to indicate that it is an immigrant from the north into South America.

In regard to ruminants, it has already been mentioned that bones of cattle from Corstopitum, Northumberland, have been regarded by Messrs. Meek and Gray as indicating a wild species identical with the white cattle of British parks for which the superfluous name *Bos sylvestris* has been proposed. Small skulls of the extinct wild ox or aurochs from the superficial deposits of Belgium have also been described by Mr. R. v. d. Malsburg (*Bull. Internat. Ac. Cracovie*, 1911, No. 5) as a new species under the name *B.* (*urus*) *minutus*. Unless, however, the term *urus* be used in a generic sense, it would seem that a sub-species is intended. Under various forms, the author regards this *Bos minutus* as the connecting link between the typical aurochs (which is considered to be the descendant of the Indian *B. namadicus*) and modern cattle.

Remains of extinct wild cattle from Bute have likewise been recorded by Mr. James Ritchie in *Trans. Buteshire Nat. Hist. Soc.* vol. iii. pp. 67-8.

The Tertiary giraffes and their allies of India form the subject of a monograph by Mr. Guy Pilgrim, published in the *Palæontologia Indica* (new series, vol. iv. No. 1). The list of Siwalik representatives of the group is a long one; and Mr. Pilgrim has brought the available information up to date. He divides the Siwalik species into brachycephalic (short-skulled) and dolichocephalic (long-skulled) types; and he has likewise been enabled to throw light on the respective geological horizons of the various forms. The most generalised Siwalik representatives of the group are described by the author under the name *Progiraffa* but as one of them was, for a time, called *Propalæomeryx* by the present writer, the latter designation is clearly entitled to stand.

By general consent the group is now taken to include the Indian *Sivatherium*, *Bramatherium*, and *Hydaspitherium* and the European *Helladotherium*; but this was not always the case and it may be recalled that, as a young man, the present writer had a hard fight against the authority of the late Prof. Rütimeyer, of Basle, to get this recognised. Rütimeyer contended that the great columnar pedicle from which the antler-like horns of *Bramatherium* and *Hydaspitherium* take origin is akin to that of the hartebeests (*Bubalis*) and consequently that the former are antelopes. That the two structures are identical in general character may be freely admitted but it does not therefore follow that they indicate genetic affinity between the animals in which they occur; as a matter of fact, they are examples of parallelism in development, a factor in the evolution of structures which had not been recognised at the date of the aforesaid discussion.

The wonderful development attained by the camel group during the Tertiary period in North America has been further illustrated in the course of the year by Messrs. F. B. Loomis and O. A. Peterson. In a paper by the former writer on the camels of the Miocene Harrison beds (*Amer. J. Sci.* ser. 4, vol. xxxi. pp. 65–70), for instance, additional characters are given of some of the extinct genera, with descriptions of three new species. The Harrison beds indicate a dry upland country, where camels were represented abundantly by species of the genus *Stenomylus* and less commonly by those of *Oxydactylus*. Both these have the complete mammalian series of forty-four teeth; the former being characterised by the tall crowns of its cheek-

teeth and its slender build, whilst the second is distinguished by the shortness of these teeth and the elongation of the limbs. So perfectly are the remains of *Stenomylus* preserved in the quarry on the Niobrara River, Nebraska, which takes its name from the genus, that it has been found practicable to mount a complete skeleton for the Carnegie Museum. This has been described in the *Annals of the Carnegie Museum*, vol. vii. pp. 267–73, by Mr. Peterson, who, on account of certain peculiarities in the skull, regards the genus as indicating a special group of the family. He also considers it to be an immigrant into the Niobrara district, where it became modified in accordance with the upland nature of its environment.

In a second communication the same writer (*op. cit.* pp. 260–66) describes a beautifully preserved skull and other remains of a new species of *Oxydactylus* from the Miocene of Western Nebraska.

The discovery in the autumn of 1907 of mummified carcases of mammoths and rhinoceroses in the ozokerit deposits of the Starunia district of Eastern Galicia gave rise to considerable interest at the time. The specimens, after being carefully disinterred, were transferred to the museum at Lemberg, where the skin of the rhinoceros has been set up and an illustrated account of both specimens, by Mr. E. L. Niezabitowski, is published in the *Anzeiger d. Akad. d. Wissenschaften in Krakau* (1911, pp. 229–39 and 240–66). The rhinoceros, which belongs to the woolly species (*Rhinoceros antiquitatis*), is so well preserved that the author has been enabled to compare the head in detail with that of its nearest relative, the existing white rhinoceros of Africa. The species presenting the next nearest relationship appears to be the European Pleistocene *R. mercki*. The hair of the Starunia rhinoceros had disappeared, probably owing to the action of the ozokerit.

This is, however, by no means the only memoir on fossil rhinoceroses, for, although dated 1910, copies of one by Dr. O. Abel on the early European Tertiary species published as part 3 of vol. xx. of *Abhandlungen der k.k. Geologischen Reichsanstalt*, did not reach this country till 1911. The author adopts the view that the rhinoceros-group should be divided into the families *Hyracodontidæ* (including *Hyrachyus* as well as *Hyracodon*), *Amynodontidæ*, and *Rhinocerotidæ*, each of which

has undergone divergent development. No fewer than four new generic terms are proposed for the early Tertiary representatives of the latter, the first two of these being based, respectively, on Cuvier's *Rhinoceros minutus* and Filhol's *Aceratherium minus*, while the other two are established on new species, one from the lignite beds of Monte Bolca and the other from the Oligocene of Krain. Special classificatory importance is accorded to the form of the last upper premolar tooth, of which the oldest type is found in *Hyrachyus*, where the two cross-crests converge on the inner side to form a U-like loop. From this may be traced a gradual transition to the modern rhinoceros-type, in which the last premolar has become like the molars, the first two of which carry a pair of completely sundered and parallel cross-crests. Progressive increase in the complexity of the upper molars themselves is likewise noticeable as we pass from the earlier to the later Tertiary species.

Dr. Anton Koch has likewise described (*Ann. Mus. Nat. Hungar.* vol. ix. pp. 371–87) certain well-preserved remains of a small and primitive rhinoceros from the Middle Oligocene of the Kolozsvār district. It is referred to the above-named species, which is now known as *Præaceratherium minus*.

In 1910 Mr. F. Roman described (*Ann. Soc. Linn. Lyon*, vol. lvi. pp. 117–30) a skull of the Miocene *Rhinoceros sansaniensis* in the Museum at Nérac, and likewise contributed a preliminary note (*C. R. Acad. Sci. Paris*, vol. cl. pp. 1558–60) on the Oligocene rhinoceroses of Europe generally. The latter subject he has further elaborated during the present year in a memoir published in vol. xi. pp. 1–92 of the *Archives* of the Lyons Museum. Two skulls, respectively belonging to *R. antiquitatis* and *R. mercki*, from the Pleistocene of Thuringia are described and figured by Mr. O. Wüst in the Stuttgart *Palæontographica*, vol. lxviii. pp. 133–88, pl. x.

In America remains of rhinoceroses with a transverse pair of nasal horns (*Diceratherium*) are found in splendid preservation in the Agate Spring Quarries of Sioux County, Nebraska, among them being a complete skeleton which has been mounted in the Carnegie Museum, Pittsburg. In describing this specimen Mr. O. A. Peterson (*Ann. Carnegie Mus.* vol. vii. pp. 274–7) remarks that it " represents an animal with a well-proportioned head, a short neck, a long and rather heavy body supported by

legs comparatively short and thick. The dentition, together with other anatomical features of the skeleton, suggests an animal whose habitat was among shrubs and vegetation along streams and other bodies of water."

Turning to the equine group, considerable interest attaches to the description by Dr. Studer, in *Verh. deutsch. Zool. Ges.* 1910–11, pp. 192–200, of a new species of three-toed horse (*Hipparion proboscideum*) from the Upper Tertiary of the Isle of Samos, characterised by the unusual development of the premaxillary bones of the upper jaw. From this feature the author believes the species in question to have been furnished with a short trunk, like that of a tapir; he is also led to the conclusion that the preorbital pit of so many extinct horses—very generally regarded as a receptacle for a face-gland or larmier—really served for the attachment of muscles connected with a proboscis, which is considered to have been developed in all the earlier members of the group.

In this place mention may be made of the genus *Hyperhippidium* established in 1910, although not referred to in my review for that year, by Mr. Sefoe (*Stockholm Vet. Ak. Handl.* vol. xlvi. No. 2), for *Onohippidium peruanum*, from the Pleistocene deposits of Peru. The genus *Onohippidium*, it will be remembered, is typified by a small horse-like animal from the Pampean formation of Buenos Aires, characterised by the great length of the nasal bones and slits together with the huge size of the double preorbital pit. The Peruvian form is stated to possess skull-characters worthy of generic distinction but I have not myself seen the original paper.

Brief mention must suffice for an article by Prof. H. F. Osborn (*Science*, vol. xxxiii. pp. 825–8) on continuity in development among the members of the extinct North American perissodactyle family *Titanotheriidæ*.

Reference has been already made to Mr. Niezabitowski's description of the mummified carcase of a mammoth from the ozokerit deposits of Galicia; and bare mention will suffice for an article by Mr. Klein (*Schr. physik. Ges. Konigsberg*, vol. li. pp. 47–55) on the occurrence of the same species in Eastern Prussia, and for a second by Mr. Harlé (*Bull. Soc. Géol. France*, ser. 4, vol. x. pp. 163–5) on the occurrence of the mammoth in le Sable des Landes, France.

Of more importance is a note by Dr. G. Schlesinger (*Monats-*

ber. Ver. Landeskunde Niederösterreich, 1911, No. 16) on the tooth of an elephant from the Pliocene of Lower Austria apparently allied to or identical with *Elephas planifrons* of the Indian Siwalik; such a westward extension of this type being of considerable interest.

In this place mention may be made of an important article by Mr. Franz Bach, published in 1910 (*Beitr. Pal. Öster.-Ung.* vol. xxiii. pp. 63–124) but not referred to in my summary for that year, on the mastodons of Styria. Dr. Toula (*Abh. Geol. Reichsanstalt, Wien*, vol. xx. pt. 5, pp. 46, 47) has likewise described and figured teeth of *Mastodon borsoni* from the Budapest and Arad districts.

The most important discovery connected with the Proboscidea is the identification by Dr. C. W. Andrews (*Proc. Zool. Soc. London*, 1911, pp. 943–5) of remains of the European and Asiatic Upper Tertiary genus *Dinotherium* in East Central Africa, since this may be the forerunner of other discoveries in regard to the extinct proboscideans of that continent.

In view of the indications of affinity between the elephant group (Proboscidea) and the sea-cows (Sirenia), to which attention was directed a few years ago by Dr. Andrews, particular interest attaches to a paper by Mr. J. C. Merriam (*Bull. Dep. Geol. Univ. California*, vol. vi. pp. 403–12) on the genus *Desmotylus*, originally described by O. C. Marsh but subsequently identified in Japan. In America the genus is now known to occur in the marine Miocene strata of California; and it may be definitely included in the Sirenia. "The peculiar characters of the skull and dentition of *Desmotylus*," observes Mr. Merriam, "add somewhat to the evidence which has been held to indicate relationship of the Sirenia to the Proboscidea."

Passing from sea-cows to cetaceans, it may be noted in the first place that important new light is thrown on the ancestry of porpoises and dolphins by Mr. F. W. True's preliminary note (*Proc. Biol. Soc. Washington*, vol. xxiv. pp. 37, 38) on a species of the extinct genus *Delphinodon* from the Miocene of Maryland. This genus, which has been hitherto referred to the *Squalodontidæ*, is regarded by Mr. True as a dolphin with tuberculate teeth. It serves to prove, in his opinion, that the *Delphinidæ* are "derived from forms having teeth with tuberculate or serrate crowns, rugose enamel and anterior and posterior longitudinal ridges." This type of tooth still survives in the modern genus

Steno. "It is probable that the earlier ancestral forms of the *Delphinidæ* were allied to *Squalodon* and that the families *Delphinidæ* and *Squalodontidæ* are offshoots from a common stem, though, from the little that is known of the vertebral column and limbs of the squalodonts, the hypothesis still appears tenable that the *Delphinidæ* originated from a stock distinct from the former but having somewhat similar tuberculate teeth."

The shark-toothed dolphins (*Squalodontidæ*) and primitive whales (*Zeuglodontidæ*) of Australia and New Zealand form the subject of an article by Dr. T. S. Hall in the *Proceedings* of the Royal Society of Victoria. It is there shown that the southern representatives of these groups differ from their northern relatives by the fuller union of the roots of the cheek-teeth. Hector's genus *Kekenodon* is stated to differ from the rest by the great size of the roots of these teeth.

The discovery of a new specimen of *Squalodon ehrlichti* in the Linzer sands is recorded by Dr. A. König in the *Jahrsbericht Mus. Franc.-Carol., Linz*, vol. lxix. pp. 109–21, and illustrated by a plate.

New forms of the South American glyptodont genus *Hoplophorus* are described by Mr. Richter in the *Palæontographica*, vol. lvii. pp. 257-83.

As usual, work relating to fossil birds has been of limited extent. Mr. L. H. Miller has, however, contributed two papers on bird-remains from the superficial deposits of California, published in the *Geological Bulletin* of California University. The first of these (vol. vi. pp. 305–15) deals with remains of eagles from the well-known asphalt deposits of Rancho la Brea, near Los Angeles; a noticeable feature being that all the species are identified by means of the shank-bone (tarso-metatarsus). The species represented include the golden eagle, the white-headed sea-eagle and extinct forms of the modern South American crested eagle (*Morphnus*) and Chilian sea-eagle (*Geranoaëtus*).

In describing the specimens the author notes that while the shank-bone of the South American harpy eagle (*Thrasaëtus*) recalls a gigantic sea-eagle (*Haliaëtus*), the corresponding bone in the other two genera is more like that of a typical eagle (*Aquila*), only of a much more elongate type.

In the second communication (*op. cit.* pp. 385–400) Mr. Miller describes a large series of bird-remains from Californian caves, some of these belonging to living and others to extinct types.

They include extinct species of American vultures of the genera *Catharista* and *Gymnogyps*, an eagle-owl, a turkey and several representatives of the peculiar existing genera of American game-birds.

It may be of interest to note that the La Brea asphalt formation has been for some years worked solely for the sake of the bones, the asphalt being so mingled with sand as to be of no commercial value. The asphalt which lies in a deep pit with a superficial area of some 30 ft. square, is constantly bubbling up and is overlain with a thin layer of water. This layer of water serves for a lure to mammals and birds which come to drink and when they have entered or alighted on the area are held fast in the tenacious asphalt by their feet till they die a lingering death by starvation, unless they are sufficiently heavy to become engulfed in the treacherous black deposit. What is going on at the present day appears to have been in progress ever since the Pleistocene period, for the asphalt is packed with skulls and bones, some of which, as noted above, belong to existing species, whilst others pertain to extinct types, such as sabre-toothed tigers and ground-sloths. So numerous are the remains of the former species that one collector is stated to possess about sixty skulls and there are doubtless scores more awaiting extraction. Skulls of a large wolf are also common. As regards the remains of eagles and vultures, it is noteworthy that the majority consists of the shank-bones (tarsi), thus indicating that the birds were held by their feet in the asphalt, while their bodies remained above the surface to be decomposed and disintegrated. By the constant movement which takes place in the asphalt, the shank-bones and toe-bones eventually become buried to a considerable depth. A death-trap of this nature seems to be unknown in any other part of the globe.

A bone from the superficial deposits of Darling Downs, Queensland, is identified by Mr. C. W. de Vis (*Ann. Queensland Mus.* No. 10, pp. 15–17) as the basal phalange of the first toe of a bird probably allied to the accipitrines. It is provisionally made the type of a new genus and species, *Palæolestes gorei.*

The well-known Jurassic bird *Archæopteryx* forms the basis of an article by Dr. O. Abel entitled "Die Vorfähren der Vögel und ihre Lebensweise" (*Verh. zool.-bot. Ges. Wien*, vol. lxi. pp. 144–91), in which the author discusses the arrangement of the

plumage in the extinct genus, the function and reduction of the great toe (hallux) in birds and dinosaurs, the opposability of the same toe in certain of the older groups of dinosaurs, the homology and relative length of the bird's finger as compared with its representative in the carnivorous dinosaurs, the function of the claw in the living hoatzin (*Opisthocomus*) of South America, the pose and mode of locomotion of the bipedal dinosaurs, the functions of the hand and foot in the oldest carnivorous members of that group, and the arboreal habits of the ancestors of both carnivorous dinosaurs and birds, which it is suggested may be known as "avidinosaurs." *Archæopteryx*, it is pointed out, appears to have had a plumage resembling in many respects that of the golden pheasant, and its flight was almost certainly of the slow heavy type characteristic of many game-birds.

The prolonged and perhaps somewhat wearisome discussion with regard to the normal pose of the limbs in *Diplodocus* and other giant sauropod dinosaurs, to which considerable space was accorded in my review for 1910, has been continued. In the latter part of that year, for instance, Mr. F. Drevermann (*Sitz Ber. Ges. Natf. Freunde Berlin*, 1910, pp. 399–401) criticised the views of Dr. Tornier as to the squatting posture of these reptiles, to which Dr. Tornier replied in the same issue (pp. 402–5). The subject was resumed in the *American Naturalist* for July, 1911, by Dr. O. P. Hay, who once more criticises the opinion that these reptiles carried themselves in elephantine fashion, reasserting his own view that the general pose was more after the crocodilian style. In regard to what may be called the elephant-pose, it is pointed out that since a straight femur appears to have characterised the proboscidean (elephant) stock from the beginning, its occurrence in the modern representatives of the group may be regarded as a primitive feature, rather than an adaptation to the support of great bodily weight. At the conclusion of his argument Mr. Hay expresses doubts as to whether the erect bird-like posture attributed to the carnivorous dinosaurs of the Jurassic is really true. "The extraordinary development of the pubic bones of *Aristosaurus*, the expanded and anchylosed distal ends of which reached nearly half-way to the fore-legs, seems to me to indicate that these animals, when in repose, had a prone position, resting much of the weight on the pubes, and that when running their

legs straddled considerably." Further remarks on this subject are recorded in the sequel.

One of the most interesting discoveries recorded during the year is that of what may truly be termed a dinosaur-mummy. As described by Prof. H. F. Osborn in the *American Museum Journal* for January 1911, the remains in question are those of a giant reptile from the Laramie Cretaceous of Wyoming which include not only the greater part of the skeleton but likewise the skin, which is tightly drawn over the bones. Other specimens have been obtained from the same formation in which portions of the skin are retained; but this is the only one in which nearly the whole is preserved, although its describer is of opinion that if due care had been taken in their extraction from the matrix several other skeletons would also have shown the dermal covering. This reptile, which stood from 15 ft. to 16 ft. in height, with a total length of about 30 ft., is known as the duck-beaked dinosaur, on account of the great lateral expansion of the toothless extremities of the jaws. Not that the creature was altogether toothless, for the sides of the jaws are armed with a pavement-like arrangement of flat-crowned grinding teeth recalling those of the iguanodons of the English Wealden but of a more specialised type. The skin of this reptile (*Trachodon insignis*), for an illustration of which I am indebted to Prof. Osborn, was covered with granules or tubercles, varying in size in different parts of the body, of which the impressions are perfectly preserved. Prof. Osborn is of opinion that the carcase of the reptile lay for some time after death on the banks of a river millions of years ago, where it was subject to the rays of a scorching sun until converted into a mummy. A flood probably then carried the desiccated carcase down-stream till it reached a point where it was retained and quickly buried beneath a mass of clayey sand sufficiently stiff to show the impression of the surface of the skin. The specimen also shows that the short fore-limbs, instead of having claws, were flattened out and covered with skin, so as to form a kind of paddle-like structure. In addition to showing a pattern in the size of the tubercles, the skin probably also exhibited a colour-pattern, so that from the analogy of lizards and snakes it is not unlikely that these giant reptiles presented a darker appearance when seen from in front than when viewed from behind. The position where the skeleton was found, coupled with the paddle-

like shape of the fore-limbs, suggests that these reptiles were largely aquatic in habits, although, like iguanodons, they habitually assumed an upright posture.

This wonderful skeleton was discovered in 1908 by the veteran collector Mr. C. H. Sternberg. One of the illustrations in a popular account published in the May number (1911) of the *Scientific American*, as well as the photograph here reproduced, show the finely tuberculated or granulated structure of the external surface of the skin, whilst a second reproduces the latest restoration of the entire animal. The tenuity of the skin confirms the idea of the aquatic habits of these dinosaurs; the expansion of the extremity of the muzzle into an edentulous, duck-like beak being likewise indicative of aquatic habits.

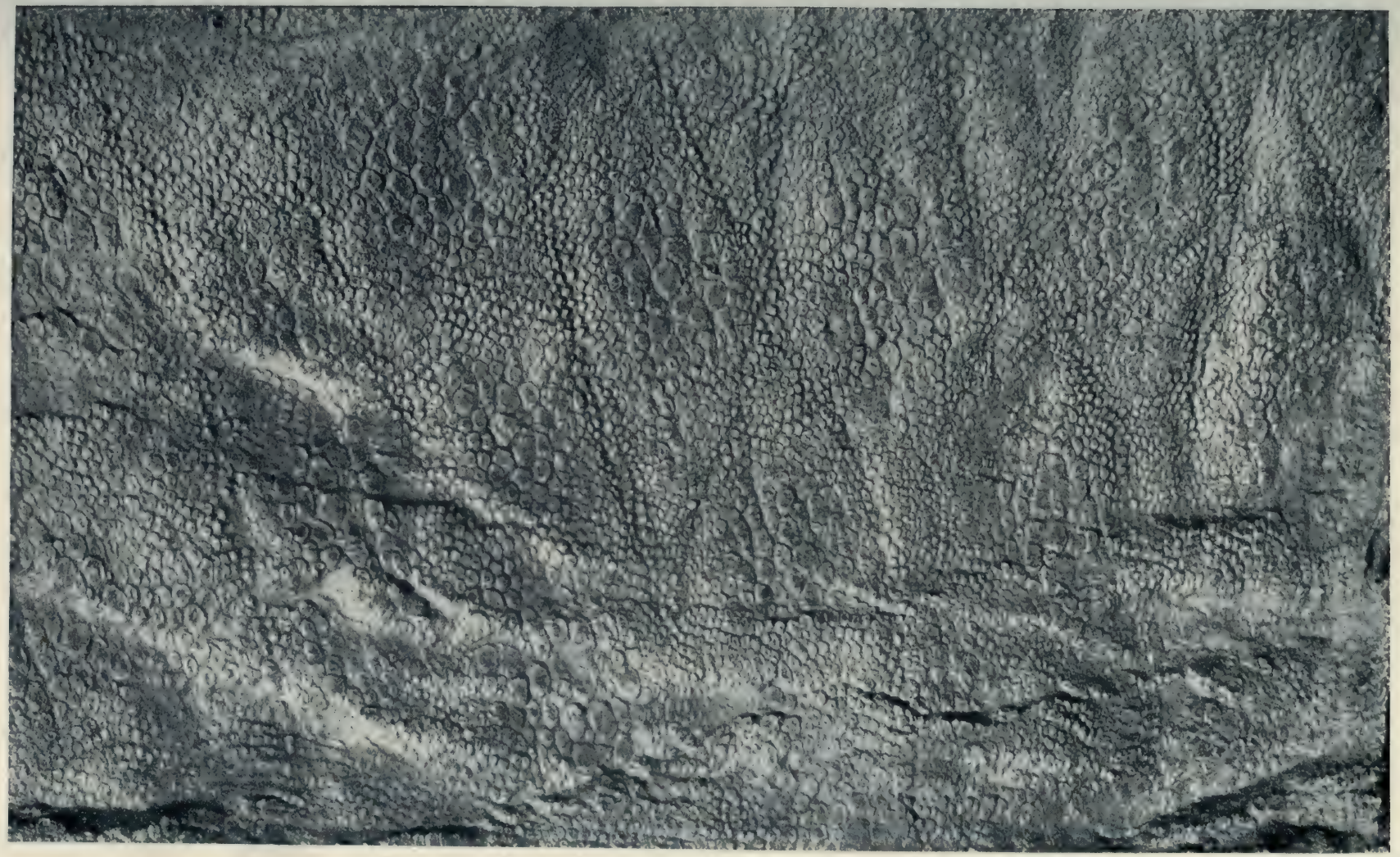
To have been enabled to obtain an idea not only of the structure but likewise of the colour-pattern of dinosaurian skin is a most wonderful and unexpected result of palæontological investigation.

Equal interest attaches to the preliminary account of the collections made by the expedition despatched from Berlin in search of dinosaurian remains from the Cretaceous strata of the Tendaguru Mountains of German East Africa. According to a popular article contributed by Prof. Branca to *Naturwissenschaftliche Wochenschrift* for April 30, 1911, the most noteworthy feature of these remains, so far as they have been examined, appears to be their gigantic dimensions, which much exceed those of *Diplodocus*. The longest rib of the latter measures, for instance, 1·86 m., whereas some of the African ribs are not less than 2·50 m. in length. Again, the longest cervical vertebra of the former is 0·64 m., in contrast to which is one from Tendaguru measuring 1·10, whilst a humerus of the African dinosaur is more than twice as long as the corresponding bone of *Diplodocus*, measuring 2·10 m. (6 ft. 10 in.) against 0·95 m. (3 ft. 1 in.). It must be remembered, however, that *Diplodocus* is by no means the largest non-African dinosaur, its thigh-bone, or femur, measuring 1·542 m., or 5 ft. 1¾ in., against 6 ft. 2 in. in that of the North American *Atlantosaurus*. Still, as the femur is frequently a longer bone than the humerus, the advantage is largely on the side of the African reptile. The African dinosaur seems, however, to be rivalled in point of size by the North American *Brachiosaurus*, in which one of the ribs measures 2·74 m. in length, while the length of the femur is 2·03 m. and that of the

humerus 2·04 m. The latter is a little less than the length of the African humerus but serves to show that the femur is not necessarily the longest of the limb-bones. *Brachiosaurus* and the African dinosaur may prove to be related. In addition to those of dinosaurs, bones apparently referable to pterodactyles have been obtained. Remains of dinosaurs have likewise been discovered in several other localities of German East Africa. Prof. Branca adds that if the sauropod dinosaurs subsisted on a vegetable diet, it is difficult to imagine how they obtained sufficient nutriment. The same difficulty, it may be mentioned, has occurred to Mr. J. Versluys, who suggested (*Zool. Jahrb., Abtheil. f. Systemat.*, vol. xxix. p. 425) that these reptiles fed on fishes. Prof. Branca is also exercised in his mind how sauropods obtained sufficient calcareous matter for their enormous skeletons; but the supply of this substance would obviously increase *pari passu* with the amount of food consumed.

In reference to the above-mentioned remarks of Mr. Versluys on the food of dinosaurs, it may be added that in the opinion of that naturalist the capacity of the abdominal cavity of these giant reptiles is not large enough to have contained the enormous amount of vegetable matter necessary for the support of such monsters. On the other hand, it is argued that the powerful muscles of the neck (as indicated by the bony projections to which they were attached), combined with the mobility of the neck itself and the relatively small head, indicate definite and rapid movement in a dense medium. Moreover, the nature of the dentition and the form of the cavity of the mouth appear to be a special adaptation for capturing living prey. And it would thus seem that these reptiles frequented the banks and borders of rivers or lakes, where they captured fish under water, which they swallowed whole without any attempt at mastication. It is further suggested that the long whip-like tail of *Diplodocus* might have served to beat the water and thus drive the fish into the shallows.

Reference was made in my review for 1910 to Dr. R. S. Lull's restoration of the skeleton of the armoured dinosaur *Stegosaurus*; this restoration Mr. G. R. Wieland (*Amer. J. Sci.* vol. xxxi. pp. 112–24) suggests is incomplete, for he claims to have evidence of the existence of a series of low, lateral, bony keels in addition to the large dorsal plates. In the



Part of the skin of the Mummified Dinosaur, *Trachodon insignis*.

From a photograph supplied by Prof. H. F. Osborn.

same communication Mr. Wieland furnishes additional particulars with regard to the dermal plates and other remains described under the name of *Hierosaurus sternbergi*, from the Niobrara Cretaceous, which, it is stated, indicate a reptile of about four metres in length and therefore about half the size of *Stegosaurus*. Possibly they may prove generically inseparable from the reptile previously named *Stegopelta*.

Hitherto the great *Omosaurus armatus* of the Kimeridge Clay of Swindon has been the only well-known European representative of the *Stegosauridæ*, and Baron F. Nopcsa's identification (*Geol. Mag.* decade 5, vol. viii. pp. 109–53) of a British representative of the typical American genus is therefore of considerable interest. The remains which were obtained from the Oxford Clay, near Peterborough, are referred to a new species, *Stegosaurus priscus*, specially distinguished by the relatively small height of the neural arches of the trunk-vertebræ.

Podokosaurus holyokensis is the somewhat uncouth designation given by Miss Mignon Talbot (*Amer. J. Sci.* vol. xxxi. pp. 469–79) to the imperfect skeleton of a small and apparently carnivorous dinosaur contained in a split boulder of the Triassic sandstone of the Connecticut Valley. The length of the body is about eighteen centimetres; the proportions of the limbs are indicative of bipedal and cursorial habits in an arid district. Possibly this dinosaur may have an earlier name, as the bones may represent one of the reptiles named on the evidence of footprints from the Connecticut Trias.

The dinosaurs of the Stormberg beds of South Africa are discussed by Dr. R. Broom in an article published in vol. vii. part 4, of the *Annals* of the South African Museum. Seven distinct generic types of these Stormberg dinosaurs are recognised by the author, namely, *Euscelesaurus*, *Hortalotarsus*, *Gyposaurus*, *Gryponyx*, *Massospondylus*, *Ætonyx* and *Geranosaurus*, of which the third, fourth, sixth and seventh are described for the first time. The first six are referable to the carnivorous group but the last is characterised by the presence of a toothless predentary bone at the tip of the lower jaw. The latter specialised feature suggests that the horizon of the Stormberg beds is of Lower Jurassic rather than Triassic age and, even so, *Geranosaurus* will be the oldest known type furnished with a predentary bone.

The angulated outer and rounded inner surface of the terminal segment of the second hind toe in *Gryponyx*, *Massospondylus* and *Ætonyx* indicate that the investing horny claw had an edge adapted for combing or scraping and it is accordingly suggested that it was employed for cleaning the skin and scales. If this be so, the dermal covering was probably unlike that of crocodiles or lizards and the scales may have been long and narrow, with intervals of soft, bare skin between them, as such a skin would require cleansing with the claw after the reptile had been hunting on muddy banks of lakes.

Some important work has been done on crocodilians during the year, Mr. C. W. Gilmore having described (*Proc. U.S. Nat. Mus.* vol. xli. pp. 297–302), under the new generic and specific name of *Brachychampsa montana*, a crocodilian skull from the Cretaceous of Montana. This specimen indicates an alligator-like species distinguished from other members of the alligator-group by the absence of a posterior expansion of the hind part of the premaxillæ to roof over the front of the nasal chamber. There are fourteen pairs of upper teeth, five of which are borne by the premaxillary or anterior jaw-bones.

Still greater interest attaches to the description by Mr. Armand Thevenin (*Annales de Paléontologie*, vol. vi. pp. 95–108, pl. iii.) of the skull of a long-snouted crocodilian from the Eocene Phosphates of Tunisia. It appears that in 1893 Mr. P. Thomas gave the name *Crocodilus phosphaticus* to a crocodilian from the same deposits, while in the following year the name *Dyrosaurus thevestensis* was proposed by Mr. Pomel for the same reptile; the generic designation being derived from the district of Dyr.. The complete skull shows that the reptile, of which the name will be *Dyrosaurus phosphaticus*, is closely allied to the Jurassic *Steneosaurus* and therefore indicates the survival of the Mesozoic type into the Eocene. The species also occurs in the Sudan and German East Africa.

Mr. H. E. Sauvage has contributed to the *Bull. Soc. Acad. de Boulogne-sur-mer*, vol. ix., a note of ten pages on the local occurrence of the giant marine crocodiles of the genus *Dacosaurus*, remains of which are common to the Kimeridgian and Portlandian of the Boulonnais.

The affinities of the Parasuchia (*Parasuchus*, *Phytosaurus*, etc.) are discussed at length by Prof. F. Huene (*Geol. u. Pal.*

Abhandlungen, vol. xiv. pt. 1, art. 2), who concludes that these reptiles form a sterile branch of the Pseudosuchia (Aëtosauria), which became separated therefrom and never had any direct connexion with the Crocodilia, although they were included in that order by Huxley.

Several important communications relating to the so-called enaliosaurian reptiles (plesiosaurs and ichthyosaurs) appeared during the year. In the first of these Dr. C. W. Andrews (*Ann. S. Africa Mus.* vol. vii. pp. 309–22) describes the skull and other portions of the skeleton of a plesiosaur (*Plesiosaurus capensis*) from the Uitenhage beds of Cape Colony. The species, which belongs to the group of small forms represented in the European Wealden by *P. degenhardti* and *P. valdensis*, is the first plesiosaur known from South Africa; and this record of the occurrence of the group in that country is of special interest, in view of the possibility that the Sauropterygia may have taken origin from a stock more or less nearly related to the therocephalous anomodonts or carnivorous mammal-like reptiles of the South African Permian.

In the second—written unfortunately in Russian—Mr. N. Bologabow describes in a memoir issued by the Geological Cabinet of Moscow University for 1911 (412 pp., 16 pls.) an extensive series of plesiosaurian remains from the Mesozoic formations of Russia. Many of these are referable to previously known forms but the author describes as new species of *Thaumatosaurus*, *Cryptoclidus*, *Muræosaurus*, *Colymbosaurus*, *Elasmosaurus* (3), *Polycotylus* (4), and *Cimoliosaurus*.

In a third communication Dr. E. Fraas,[1] when describing (*Mitt. Kgl. Nat.-Kabinett Stuttgart*, No. 77) a finely preserved skeleton of an embryonic or newly born specimen of *Ichthyosaurus quadriscissus* from the Upper Lias of Holzmaden, Württemberg, in which the outline of the body and fins is preserved, gives an account of the evolution of the tail-fin in the ichthyosaurs. That the specimen on which this investigation was based is extremely young is rendered evident by the great relative size of the head, which is equal to one-third the entire length, whereas in the adult it is less than one-fifth. The earliest known type of tail-fin in the ichthyosaurs occurs in *Mixosaurus nordenskjoeldi* from

[1] The abstract of this paper (as is also the case in several other instances) is taken, with certain modifications and by the editor's permission, from one communicated by myself to *Nature*.

the Muschelkalk of Spitzbergen, in which, according to a figure given by Mr. C. Wiman (*Bull. Geol. Inst. Upsala*, vol. x. 1910), it takes the form of a low falcate fin in the basal third of the tail, continued as a marginal expansion along the whole of the rest of the tail, both above and below. In the young of *I. quadriscissus* the falcate portion and the marginal fringe are increased in height so as to form a fin approximating to the unsymmetrical or heterocercal type of fishes; with the extremity of the vertebral column bent down into the lower moiety. In the adult of the same species the increase in the size of the fin and the degree of flexure of the vertebral column are intensified, thus producing an approximation to the symmetrical or homocercal type of the fish. A further stage is displayed by *I. trigonus posthumus*, of the Upper Jurassic Limestone of Solenhofen, in which the fin is almost completely like that of a homocercal fish, while the flexed terminal portion of the vertebral column has diminished in relative proportion, with a marked increase of the angle of flexure. One step more and it would have vanished. A somewhat similar grade is presented by the broad-paddled *Opthalmosaurus* of the Oxford and Kimeridge Clays. It should be added that in the hind paddle of *Mixosaurus* the metatarsal and phalangeal bones retain an elongated form and do not form a mosaic-like structure like that found in the later genera, thus indicating a much less wide departure from the ordinary type of reptilian limb.

Closely connected with the foregoing is an article by Mr. J. C. Merriam (*Bull. Dep. Geol. Univ. California*, vol. vi. pp. 317–27) on the relationship of the marine saurians from the Trias of Spitzbergen described by Mr. Wiman in the communication cited above. Several of these pertain to the ichthyosaurian genus *Mixosaurus* and afford important evidence in regard to the structure of the limbs and fins in the members of that genus. On the other hand, Hulke's *Ichthyosaurus polaris* is made the type of the new genus *Pessosaurus*, which appears related to *Shastasaurus* of the American Trias.

Of greater interest are certain jaw-bones with rows of button-like teeth which have been made the type of the genus *Pessopteryx*. These jaw-fragments appear akin in structure to a specimen from the Middle Trias of Nevada which has been described as *Omphalosaurus*; and it is noteworthy that the types of both these genera were accompanied by limb-bones and

vertebæ which might have belonged to a primitive ichthyosaurian. Messrs. Merriam and Byrant, in a separate communication (*op. cit.* pp. 328–32), hesitate, however, to regard the teeth and bones as associated. As regards the probable affinities of the Nevada reptile, these writers remark that "of the reptilian forms the rhynchosaur *Hyperodapedon* has several rows of depressed conical teeth on the maxillary and palatine bones. They are quite different in size and apparently in function, whilst the skull-characters of the two forms are far apart. Whatever the relationships of *Omphalosaurus* may prove to be when the skeleton is better known, the dentition is evidently a peculiarly specialised one which has been adapted to a diet of which shell-covered molluscs formed an important part."

Mr. Merriam emphasises the affinity of the Spitzbergen Triassic reptile fauna to that of central and southern Europe and that of western North America; *Mixosaurus* being common to Italy, Spitzbergen and Nevada, whilst, as already indicated, *Pessopteryx* and *Omphalosaurus* may likewise prove to be identical. Reverting to undoubted enaliosaurs, mention may be made of Mr. J. C. Merriam's description in 1910 (*Bull. Dep. Geol. Univ. California*, vol. v. pp. 381–90) of a primitive genus of ichthyosaurs (*Phalarodon*) from the Nevada Trias.

In the same year Mr. D. M. S. Watson published a further contribution to his account of British Liassic reptiles (*Mon. Manchester Lit. Phil. Soc.* vol. liv. No. 11), dealing in this instance with *Plesiosaurus propinquus.*

There is also a paper by N. Bogolubow on the occurrence of *Ichthyosaurus portlandicus* in Russia, published in *Bull. Ac. Imp. Sci. St. Pétersbourg* for 1910, pp. 469–76.

Bare mention of the title, "Les Ichtyosauriens des Formations Jurassiques du Boulonnais," must suffice for a paper by Mr. H. E. Sauvage published in *Bull. Soc. Académique de Boulogne-sur-mer*, vol. ix., as it contains nothing of special interest or importance.

Equally brief notice must be accorded to Dr. A. Misuri's account (apparently reprinted from *Atti. Ac. Sci. Veneto-Trent* for 1911) of a new species of soft river-tortoise from the Miocene of Belluno, for which the name *Trionyx vellunensis* is proposed.

In the course of an article published in No. 10 of the *Annals* of the Queensland Museum, Mr. C. W. de Vis describes two

skulls from Queensland which he refers to a turtle from the same State described by Sir R. Owen in 1882 and now known as *Notochelone costata*. That the larger of these is correctly identified seems a practical certainty; and it serves to indicate that the extinct genus is nearly related to the existing forms. The smaller skull, however, is described as having sclerotic bones in the socket of the eye—a determination which, if correct, would at once show that the specimen is not chelonian at all. It has, moreover, projections on the margins of the jaw which have the appearance of being teeth.

By far the most interesting chelonian described during the year, however, is one from the Wealden of Westphalia, for which Mr. T. Wegner proposes (*Palæontographica*, vol. lxviii. pp. 105–32, pls. viii.–x.) the new generic and specific name *Desmemys bertelsmanni*. This chelonian belongs to the extinct *Thalassemydidæ* or primitive marine turtles; but so peculiar are some of the features presented by its shell, that the author is induced to review the whole question of the relationships of that group and in the course of the discussion considers whether the *Thalassemydidæ* ought to be regarded as the ancestral type of all other members of the order, as well as whether modern chelonians pass through a thalassemyd stage in the course of development.

As being of considerable interest, reference may be made to a paper by Mr. D. M. S. Watson, published in 1910 (*Rep. Brit. Assoc.* for 1909, pp. 155–8), on a skull of the rare Triassic rhynchocephalian *Rhynchosaurus articeps*, preserved in the Manchester Museum.

Another paper published in 1910 is one by Mr. N. Bogolubow on remains of extinct sea-serpents or mosasaurians from the Cretaceous of the government of Orenburg, Russia. It appeared in the *Bull. Ac. Imp. Sci. St. Pétersbourg* for 1910, pp. 8–14. Dr. F. von Huene has also described (*Neues Jahrbuch für Mineralogie*, 1911, vol. ii. pp. 48–50) a finely preserved skeleton of the American *Platecarpus coryphæus* recently acquired by the Tubingen Museum.

Prof. S. W. Williston has again devoted much attention to the reptiles of the American Permian, his first communication during the year being one on a new family type of reptiles from the Permian of New Mexico, published in the *American Journal of Science* (vol. xxxi. pp. 378–98). This reptile, for

which the name *Limnoscelis paludis* is suggested, was evidently aquatic or subaquatic in habits—as is indicated by the extremely imperfect ossification or even wholly cartilaginous condition of the wrist and ankle joints—and had the whole of the lower surface of the body protected by a bony panoply. Its nearest relationships are evidently with the contemporary genus *Diadectes*, which is one of the best-known representatives of the cotylosaurian order. Among the distinctive differences from that genus are the longer and smoother skull, with its parietal foramen of inferior calibre, the absence of expansion in the ribs, which appear to be single-headed throughout and the aforesaid slight ossification of the wrist and ankle. The teeth, moreover, are of a conical and prehensile type. In the opinion of the author, it seems probable that the American *Diadectes* and *Limnoscelis* will eventually have to be classed with the Old World *Pariasaurus* and its near relative *Propappus.*

The whole subject of American Permian vertebrates is more fully elaborated by the same writer in a volume published under that title by the University of Chicago. In this work Prof. Williston reviews all the known representatives of the Amphibia and Reptilia included in this fauna. The reptiles he divides into (1) Cotylosauria, with the families *Diadectidæ*, *Limnoscelidæ*, *Seymouriidæ*, and *Pariotichidæ*, and (2) Theromorpha, which include the *Sphenœodontidæ* (*Clepsydrops*), *Poliosauridæ* and *Caseidæ*. The inclusion of these families among the Theromorpha or mammal-like reptiles is a wide departure from the classification adopted by many palæontologists, some of whom place *Clepsydrops* and its relative *Naosaurus* in the Rhynchocephalia.

To a great extent the classification adopted by Prof. E. C. Case in the quarto volume published by the Carnegie Institution of Washington under the title of *A Revision of the Cotylosauria of North America* agrees with that followed by Prof. Williston; the *Pariotichidæ*, *Pariasauridæ* and the *Procolophonidæ*, which many authorities class in the mammal-like group, being included in the Cotylosauria. That ordinal group is taken, in fact, to comprise all those primitive reptiles in which the temporal region of the skull is roofed with bone, the ribs single-headed and the neural arches of the vertebræ low and broad, with short spines and swollen sides. The order, which is less primitive than at first supposed, although more primitive than

any other reptiles, is believed to have originated during the Carboniferous in North America, whence it spread over the world in the Permian.

The opinion of Dr. Broom (referred to in my review for 1910) that primitive cotylosaurians and pelycosaurians originated in South America and then spread in one direction to South Africa and in the other to North America, is adversely criticised, the author maintaining the view that the North American representatives of these groups are autochthonous. It is added that the smaller and less well-known cotylosaurians will probably yield the connecting links between reptiles and amphibians.

In his table of classification Mr. Case includes in the family *Pariotichidæ* the genus *Ectocynodon* of Cope on the ground that it differs from the typical *Pariotichus* by the sculptured roof of the skull; but Mr. E. P. Branson has shown (*Journ. Geol.* vol. ix. pp. 135–9, pl. i.) that the supposed lack of sculpture in the latter genus is due to the imperfection of the material on which it was based.

In a paper published in the Zoological Society's *Proceedings* for 1911 (pp. 893–925) Dr. R. Broom supplies additional evidence of the resemblance between the skulls of the theromorphous or anomodont (in the wider sense of that term) reptiles of South Africa and those of mammals; the carnivorous cynodont group being the one in which the mammalian resemblances are most conspicuous. "If the cynodonts," observes the author of this paper, "are not nearly related to mammals, the group is still of great interest as showing a marvellous parallelism with the mammals in skull, teeth, girdles, limbs and digits; but if, as all recent work seems to indicate still more clearly, the mammalian ancestor was probably a cynodont, the group becomes vested with an interest altogether unique and everything bearing upon it becomes worthy of the most careful study."

Later on Dr. Broom, after referring to the fact that the Lower Permian pelycosaurians of North America exhibit certain mammalian features, remarks that these become more pronounced in the therocephalians and typical anomodonts of the Middle Permian of South Africa. "For the first time we get a dentition clearly divided into incisors, canines and molars; for the first time we get a lower jaw with a dentary bone which has a large coronoid process. We get a zygomatic arch

formed on the mammalian type and we lose for the first time the quadratojugal. We also get most marked mammalian characters in the postcranial skeleton. . . .

"Almost all the characters in which the cynodont skull differs from the therocephalians are characters met with in mammals. Of these the most noteworthy are: (1) formation of a secondary palate; (2) vomer very large, extending forward as a support to the secondary palate; (3) great reduction or complete loss of prevomers (*Bauria*); (4) loss of the postfrontal bone; (5) great reduction or loss of the pineal foramen; (6) two occipital condyles; (7) reduction of the quadrate; (8) a large alisphenoid bone instead of the homologous rod-like 'epipterygoid' or columella cranii of the therocephalians and anomodonts; (9) pterygoids not extending back to the quadrates, the posterior extension being replaced by the alisphenoids; (10) reduction of the angular and surangular and greater development of the dentary [bones of the lower jaw]."

In a second communication Dr. Broom (*op. cit.* pp. 1073–82) describes a number of new forms of these mammal-like reptiles from the Permian of Beaufort West, four of them being made the types of new genera. One of the most interesting of the latter (*Diælurodon whaitsi*) resembles Owen's genus *Endothiodon* in having a numerous series of columnar teeth the surfaces of which form a kind of pavement on the sides of the jaw; it is also furnished with a large pair of upper tusks like those of a dicynodon.

The remarkable and gigantic South African Triassic or Permian reptile named *Erythrosuchus africanus* forms the subject of an elaborate memoir by Mr. F. von Huene (*Geol. u. Pal. Abhundlungen*, vol. xiv. pt. 1, art. 1), by whom it is regarded as representing a distinct ordinal group, the Pelycosimia. This group is considered to be a derivative from the pelycosaurian type, in which openings were developed in the temporal region of the skull, which migrated to Gondwanaland (*i.e.* Indo-Africa).

Turning to the amphibian stegocephalians, it has first of all to be mentioned that in vol. xxxix. pp. 489–93 of the *Proceedings* of the U.S. National Museum, Mr. R. L. Moodie has described a species from the Kansas Coal Measures which is of interest on account of the rarity of such remains in that formation and also from its own intrinsic characters. This labyrinthodont, for which the new genus and species *Erpetosuchus kansensis* is pro-

posed, differs from its relatives in the presence of a pair of oval vacuities, on each side of the inner wall of the lower jaw, comparable to those of a crocodile, whilst it is further distinguished by the uniform character and shortness of its teeth. It is assigned to the family *Labyrinthodontidæ*. Classical scholars will assuredly regret that the genus was not named *Herpetosuchus*.

In the same serial Mr. Moodie (*op. cit.* vol. xl. pp. 429–33) records a third specimen of a stegocephalian salamander (*Eumicrerpeton parvum*) from the Carboniferous of Illinois in which the intestinal tract is preserved. The new specimen is larger and more developed than either of the two previously described. It is somewhat remarkable that all three appear to be females and that not one shows any traces of branchiæ. In the new specimen the intestine is longer and more convoluted than in the others, lying in five longitudinal folds and ending in a cloaca, near which are impressions of two glands, provisionally regarded as the termination of oviducts. *Eumicrerpeton* is a member of the group Branchiosauria. In this communication Mr. Moodie likewise describes a microsaurian from the same formation, referred to the genus *Amphibamus*, as *A. thoracatus*.

In Europe a new species (*B. tener*) of the genus *Branchiosaurus* from the Rothliegendes of North-Western Saxony has been described by Mr. G. Schönfeld in the *Abhandlungen Ges. Isis* for 1911, p. 19. It is specially characterised by the gills persisting at least to a late period of life, by the slenderness of the bones of the middle of the roof of the skull, the presence of teeth on the vomers and palatines and in the young state on the pterygoids, likewise by the small scales being marked with a radiating sculpture and with concentric growth-lines.

In connection with the above-mentioned paper on the ichthyosaurs of the Spitzbergen Trias, it is interesting to note that Mr. C. Wiman (*Bull. Geol. Inst. Upsala*, vol. ix. pp. 34–40) has likewise recorded labyrinthodont remains from the same locality and formation; thereby proving that this remote northern island enjoyed at that date a comparatively mild climate.

One of the most important pieces of work of the year is the systematic and morphological survey of the stegocephalians of the New York district, forming the chief part of Prof. Case's Revision of the Amphibia and Pisces of the Permian of North America, published as a quarto volume of 179 pages, with a large number of plates, by the Carnegie Institution of Washington. The

author commences with a historical review of the acquisition of the present state of our knowledge of the Permian stegocephalians and fishes, giving notices of the various schemes of classification which have been from time to time proposed and concludes this introductory review with the arrangement he himself adopts for the Amphibia. With the exception of the single genus *Lysorophus*, which is referred to the same ordinal group (Urodela) as modern salamanders, all the Permian forms are included in the Stegocephalia, which is divided into Microsauria and Temnospondyli. Most of the members of the latter are included in the rhachitomous section characterised by the bodies of the vertebræ being composed of one basal and two lateral pieces; but *Cricotus* and *Cricotillus*, in which the vertebral bodies are solid but only every other one carries a neural arch, in accordance with Cope's original scheme form a second or embolomerous, section.

The systematic and the morphological reviews of the Permian Amphibia follow this introductory survey. Prof. Williston's volume, to which reference was made when discussing reptiles, also treats of some of these Permian stegocephalians.

Two specially important memoirs on European fossil fishes have been issued during the year. It appears that in 1903 the Carnegie Museum at Pittsburg acquired the famous collection of fishes from the Upper Eocene of Monte Bolea, Italy, made by Baron Ernst de Bayet, of Brussels, sometime secretary to H.M. the late King Leopold. This collection forms the subject of a descriptive illustrated catalogue, drawn up by Mr. C. R. Eastman and published as No. 7 of the fourth volume of the *Memoirs* of the Carnegie Museum. The second of the two memoirs, which is by Dr. R. H. Traquair, deals with the fishes of the Wealden formation of Bernissart, Belgium, famous for its yield of skeletons of the iguanodon. At the conclusion of his descriptive account the author contrasts the fish-fauna of the Bernissart Wealden with that of the corresponding English formation. The most striking difference between the two is to be found in the abundance of sharks and rays in the latter and their complete absence from the former. That this is not due to incomplete knowledge of the Belgian fauna is rendered evident by the fact that no less than 2,927 specimens from Bernissart were examined by Dr. Traquair. The explanation seems to be that the Belgian Wealden was

deposited solely in fresh water. The only fish common to the two deposits is *Oligopleurus vectensis.*

One other notable paper on European fishes has come under my observation, namely a description by Mr. Eastman of new sharks and rays from the Jurassic of Solenhofen, in Bavaria, as typified by specimens in the Carnegie Museum at Pittsburg. The most interesting of the new forms named in this paper (*Amer. J. Sci.*, vol. xxxi. pp. 399–404, pls. i.–iii.) are the shark *Cestracion zitteli* and the ray *Squatina menor.* Two papers relating to the discoveries of fossil fish-faunas in Africa are of more than ordinary interest. In the first of these Mr. Leriche (*C. R. Ac. Sci. Paris*, vol. cli. pp. 840, 841) records the first find of extinct fishes in the Lualaba basin of the Belgian Congo. In the second Dr. A. Smith Woodward (*Ann. Natal Museum*, vol. ii. pp. 227–31) notices scales collected by Dr. F. Hatch in the Ecca Shales near Ladysmith. Imperfect as they are, these remains are of interest as affording further evidence of the existence of an abundant fauna of *Palæoniscidæ* in Southern Africa during early Mesozoic and late Palæozoic times. Scales of the same general type have been obtained previously from the Karu formation of the Cape and the Permo-Carboniferous of Rhodesia and Nyasaland. The last-named were referred by Dr. Traquair in the *Quart. Journ. Geol. Soc.* for 1910 to the genus *Colobodus*, while the Rhodesian and the new Natal specimens represent *Acrolepis*, of which there may be two species.

Some interest also attaches to the description by Mr. D. von Hausemann (*Sitz Ber. Ges. Naturf. Freunde, Berlin*, 1910, pp. 387, 381) of the fossil tooth of a shark of the genus *Carcharodon* from a country so far north as Spitzbergen. In this connexion may be noticed a paper by Mr. A. Karpinsky (*Bull. Ac. Sci. St. Pétersbourg*, 1911, pp. 1105–21) on those curious spiral tooth-like structures referable to the genus *Helicoprion* and the other members of the *Edestes* group.

When discussing the work on amphibians reference was made to Prof. Case's review of the classification of the fishes in his Revision of the Amphibia and Pisces of the Permian of North America, published by the Carnegie Institution. This volume also includes an account of the Permian fishes by Mr. Louis Hussakof. From this it appears that between the years 1875 and 1894 Cope described twenty-two species, referable to ten genera, from the American Permian. Revision and

fresh material have shown that some of these are synonymous; on the other hand, a few new species have been added to the list, with the result that the present number is still twenty-two, although these represent fourteen generic types. After a systematic survey of the species, Mr. Hussakof contrasts the fishes of the American with those of the Bohemian Permian; this comparison of the faunas of these areas reveals the fact that "while the groups represented in the two are, with the exception of the Acanthodii, the same, there is a marked difference in the genera respectively represented, thus proving the long segregation of the two stocks from which the Permian faunas of the two localities are descended. The most remarkable difference between the faunas is the presence of Acanthodii (three genera) in Bohemia and their absence in Texas. The Acanthodii occur in Bohemia at the same level as *Pleuracanthus*, a horizon which may be correlated approximately with the Texas and Illinois horizons."

SOME CONSEQUENCES OF GRAHAM'S WORK[1]

THE NATURE OF ELEMENTS—THE DIFFUSION OF LIQUIDS

SCIENTIFIC workers not infrequently receive gifts of books from authors or publishers. The practice is a most praiseworthy one. It is not often, however, that the service rendered by such gifts can be traced: I am glad therefore of an opportunity to testify to their value. In 1876 I received a big book with the compliments of Angus Smith and James Young—none less than the monumental volume prepared by Angus Smith, I believe at the expense of Young, of Graham's Chemical and Physical Researches: a reprint of his published work with an all too short notice of appreciation by Angus Smith. That book has occupied a prominent place on my shelves until the present day and I have often taken it down to ponder over some part of the work described in its pages. The value of this particular gift to me individually has been very great. Indeed, the interest I have always taken in solutions and the special attention I have paid to the subject of late years is largely traceable to the inspiration I derived in early years from the study of the record of Graham's altogether simple but none the less classical experiments on diffusion.

I am glad that to-day I can return thanks in public to James Young and Angus Smith and proclaim the special value of a memoir such as they have provided. "Collect the best *monuments* of our friends, their own images in their writings," are words used by Pope: after all, a man's work *is* the one and only true monument of his worth that can be erected.

But I was led to take an interest in Graham's work at a date considerably earlier than 1876. I had the good fortune to study chemistry in pre-examination days; when it was possible to learn things in an honest way, without fear of being ruined for life by the over-teaching and spoon-feeding which under our

[1] The Graham Memorial Lecture delivered before the Royal Philosophical Society of Glasgow, January 16, 1912. Reprinted from the *Proceedings* of the Society.

commercialised system of education now so often pass as training. I was therefore allowed and encouraged to learn to help myself and to think for myself, not overweighted with text-book knowledge. When I was little more than a year old as a student of chemistry, Frankland, my trainer—I use the word trainer advisedly in place of teacher—paid me the signal compliment of setting me to work out the method of determining the organic matter in drinking water an account of which was published in our joint names in 1868. I have never forgotten his coming to me one day and suggesting that we should carry on combustions in vacuo with the aid of the Sprengel pump—using this in the way Graham had used it—and how he referred me to the paper by Graham published in 1866 on the absorption and dialytic separation of gases by colloid septa—that in which the action of a thin sheet or septum of caoutchouc in separating oxygen from air is described. About this time also, a student-friend, W. C. Roberts—the late Sir Wm. Chandler Roberts-Austen, who lectured to you on the diffusion of metals in 1900—went as assistant to Graham, who was then chemist at the Royal Mint; I thus became directly interested in Graham's work at an early stage in my career.

Fortunately I am absolved from dealing with Graham on the personal side. One of my predecessors as Graham lecturer, Dr. Thorpe, when Professor of Chemistry at Anderson's College here, delivered in March 1887 the third of these lectures to your Society and in his lecture and one delivered previously at Leeds in 1877 gave an account of Graham's life and work which is characterised by the lucidity, grace of style and completeness noticeable in Sir Edward Thorpe's writings; the lectures are published in his *Essays in Historical Chemistry*.

Prof. Thorpe's discourse to you was on "Certain modern developments of Graham's ideas concerning the constitution of matter." He speaks of Graham as a man who—as no man before him had done—dedicated his life to the study of atoms and atomic motion and quotes Angus Smith as saying:

"In all his work we find him steadily thinking on the ultimate composition of bodies. He searches after it in following the molecules of gases when diffusing; these he watches as they flow into a vacuum or into other gases and observes carefully as they pass through tubes, noting the effect of weight and of composition upon them in transpiration. He follows

them as they enter into liquids and pass out and as they are absorbed or dissolved by colloid bodies such as caoutchouc; he attentively inquires if they are absorbed by metals in a similar manner and finds the remotest analogies, which by their boldness compel one to stop reading and to think if they be really possible. . . .

"If we examine his work on Salts and on Solutions we have a similar train of thought. . . .

"A chemist must take great pleasure in following Graham when he seeks the laws of the diffusion of liquids and traces their connections, especially when they lead to such results as he expressed by dialysis, a process founded on a new classification of substances and promising still the most valuable truths. We see in the inquiry how Graham thought on the internal constitution of bodies, by examining the motion of the parts and from the most unpromising and hopeless masses under the chemist's hands—amorphous precipitates of alumina or of albumen—brought out analogies which connected them with the most interesting phenomena of organic life. Never has a less brilliant-looking series of experiments been made by a chemist, whilst few have been so brilliant in their results or promise more to the inquirer who follows into the wide region opened."

No better summary of Graham's labours could well be given. In one of his passages Prof. Thorpe compares Graham with Dalton:

"The tendencies of both men," he says, "were essentially introspective. Each was capable of the most patient and concentrated thought and of steady, prolonged attention, wholly abstracted from external objects and events." He adds: "I have heard the late Dr. Young narrate the most extraordinary instances of Graham's power of mental abstraction. Dalton said of himself that 'If I have succeeded better than many who surround me, it has been chiefly, nay, I may say, almost solely from unwearied assiduity. It is not so much from any superior genius that one man possesses over another but more from attention to study and perseverance in the objects before them, that some men rise to greater eminence than others.'

"It seems like a contradiction in terms, when we reflect for a moment upon the characteristic features and tendency of his work, to say that Graham, like Dalton, was utterly devoid of the quality we call imagination. Henry says of Dalton that imagination had absolutely no part in his discoveries, except, perhaps, as enabling him to gaze in mental vision upon the ultimate atoms of matter and as shaping forth those pictorial representations of unseen things by which his earliest as well as

his latest philosophical speculations were illustrated. Graham would not even allow his fancy that amount of play. Even in the speculative essay from which I have quoted so largely, it seems as if every word had been weighed and every sentence put together with slow, laborious thought. This passionless aspect of his work seems to have greatly impressed Angus Smith, himself a man of lively sympathy and quick susceptibility. 'His works,' says Smith, 'are full of care but not of joy.'"

I am not prepared to agree either with Dalton or with Prof. Thorpe. Men of genius may easily underrate themselves and depreciate their work—as Dalton assuredly did his own powers in the words I have quoted. And however true it may be that some rise to greater eminence than others by attention to study and by perseverance, eminence is not necessarily proof of superior genius. Perhaps more often than we are aware the genius arrives at a conclusion intuitively and instantly: he no sooner has an idea than he has proved it to his own satisfaction; the laboured proof he puts forward subsequently is in deference to public opinion, either to convince others less gifted with insight or that they may appreciate his train of thought. The word genius has an altogether special connotation, to my thinking; unless my reading of Graham's work be quite wrong, judging from its wonderful completeness and logical consistency, I should rate it as full of imagination: no man without imagination could have carried through a connected series of researches like Graham's with regard to all the states of matter—gaseous, liquid, solid, crystalline and colloid. But his imagination was so sound, so true, that it enabled him to curb his mental activity and prevented him from appearing to pass outside his facts. When we consider how little beyond what he established we know and understand of the intricate phenomena of diffusion, his clearness of perception may well be regarded as surprising. I seem to see more than care in his work—passion is there also but held severely in check; achievements such as his *must* have given the fullest satisfaction to their author.

After all—who shall say what is in a man's mind if he do not care to disclose himself? As to Henry's reading of Dalton's, probably it was impossible for Henry to understand Dalton: he was not big enough; every biographer has this difficulty: Carlyle dwells on it, on many occasions, in discussing the relation of hero to valet; and no better example could be given

than is afforded by a recent attempt to reduce the lives of Great Men to rule.

1. The Nature of Elements

Graham was a believer in the essential unity of matter. Five-and-twenty years ago, Thorpe could only assure you that although the elements remained elements, chemists were with Graham in spirit; taking all the evidence into account, he said: "The mind insensibly appeals to the principle of continuity and refuses to believe that the seventy and odd 'elemental' forms to which our processes of analysis have reduced all the kinds of matter we see around us differ in essence from bodies which are known to be compound."

But the world moves rapidly in these times—we are now returned to the days of Alchemy and talk glibly of the transmutation of elements, though we concern ourselves rather with destruction and the degradation of the expensive into the valueless than with the conversion of base into noble. It may be worth while to consider the nature of the evidence and in so doing to exercise some of the care which, we are told, is obvious in Graham's work.

About the year 1894 Lord Rayleigh, in studying atmospheric nitrogen, came to the conclusion that it was heavier than it should be if it consisted of the pure gas: in collaboration with Professor Ramsay, he subsequently separated from air the element argon, a gas which is heavier than nitrogen in the proportion of about 20 to 14. Soon afterwards Ramsay discovered the element helium in the mineral *Cleveite*; at a later date he separated three other new gases from air, neon, krypton and xenon. Except argon, of which air contains about 1 per cent., these gases are present in extraordinarily small proportions:

	Molecular weight.	Volume in 100 volumes of air.
Helium	4	0·0004
Neon	20	0·0012
Argon	40	0·937
Krypton	83	0·009
Xenon	130	0·001

The new elements are all worthless, chemically speaking, being uniformly irresponsive to all attractions the chemist can offer them. There is only one thing we can do with them—we

can excite them electrically and make them glow in various colours, some of them in more than one way; the spectra they afford are not only very beautiful and characteristic but remarkably complex.

The picture we form of a gas—mainly in consequence of Graham's researches on gaseous diffusion—is that of a perfect hail-storm of infinitely small shot flying with great velocity in every possible direction, perpetually hitting up against one another and against the walls of the containing vessel—such impacts being the cause of the pressure the gas exerts on the surfaces exposed to bombardment by its molecules, as the flying particles are termed.

The great generalisation known as Avogadro's theorem—embodied in the axiomatic statement that under the same conditions of temperature and pressure equal volumes of gases contain equal numbers of molecules—enables us to assert that the weights of the molecules of gases are as the weights of equal volumes of the gases at the same temperature and pressure, that is to say, as their relative densities. Thus as oxygen gas is sixteen times as dense as hydrogen gas, molecules of oxygen must be regarded as sixteen times as heavy as molecules of hydrogen, whatever the actual dimensions of the molecules may be. But the molecules even of elements in perhaps most cases are to be regarded as chain shot—not as whole shot or shell—that is to say, as divisible into atoms. Thus a molecule of hydrogen and a molecule of oxygen is each composed of two atoms: moreover, as we assign unit weight to the hydrogen atom and take hydrogen as the unit of relative gaseous density, the conclusion that the hydrogen molecule consists of two hydrogen atoms, each of unit weight, involves the adoption of the rule:

Relative gaseous density × 2 = molecular weight.

Obviously, if the relative weight of the atom be known, it is only necessary to divide that of the molecule by that of the atom, to obtain the number of atoms in the molecule—in other words, the complexity of the chain shot. We cannot ascertain the degree of "chain-shottedness" of an elementary molecule, however, unless we know the atomic weight of the element—and this can only be determined from the study of compounds of the element, the atom being the combining unit.

As compounds of argon and its companions in air—let me call them the Argonides—are unknown, we can only determine the molecular weights of these elements, not their atomic weights. Ramsay contends that the argonides are all monatomic elements—that their molecules consist of single atoms; *i.e.* that the atomic weight is identical with the molecular weight. He bases this conclusion on the relatively low molecular heat capacity of these gases at constant volume. I took exception to the argument when Rayleigh and Ramsay brought their discovery of argon before the Royal Society in 1895,[1] pointing out that however much it might be held to favour such doctrine, it in no way sufficed to justify the acceptance of the view. But apparently chemists do not think for themselves in these days any more than ordinary folk do: the conclusion is now almost universally accepted that the argonides are monatomic elements.

My attitude with regard to this question is that of the Hon. Robert Boyle in the *Sceptical Chemist*:

"I blush not to acknowledge that I much less scruple to confess that I doubt when I do so than to profess that I know what I do not: and I should have much stronger expectations than I dare yet entertain to see philosophy solidly established, if men would more carefully distinguish those things that they know from those that they ignore or do but think and then explicate clearly the things they conceive they understand, acknowledge ingenuously what it is they ignore and profess so candidly their doubts, that the industry of intelligent persons might be set on work to make further inquiries and the easiness of less discerning men might not be imposed on."

The question I raise is one of fundamental importance but a difficult one to argue.

To my way of thinking, an inert element is a contradiction in terms—but in saying this I am merely giving expression to my personal sentiment.

A difficulty that is more generally felt is that of classifying the argonides in any satisfactory manner. Since the introduction of Mendeléeff's Periodic System of Classification, chemists have always recognised that the properties of the elements rise and fall in periods—it has long been clear, however, that it is impossible to order elements simply in accordance with their atomic weights. The system we are gradually being led

[1] *Chemical News*, vol. lxxi. p. 61.

to adopt is a compromise and a combination of two forms of classification—the one involving the arrangement of elements in the order of their atomic weights, the other their arrangement in families. The latter mode is of special importance because, the record being incomplete, we need some means of apportioning blanks—this is afforded by the existence of families of elements, a fact of entirely crucial importance.

As to the method followed, suppose a surface to be formed by wrapping a band spirally around a cylinder and that the cylindrical surface is divided into sections by lines drawn vertically parallel with the axis of the cylinder; if the surface be cut along one of the vertical lines and spread out, a sheet or table is obtained. In such a table a series of compartments in succession is provided; each of these will serve for an element.

Writing the symbols of the elements in succession in the order of the atomic weights—

H	Li	Be	B	C	N	O	F
1	7	9	11	12	14	16	19

—a series of diverse elements is obtained but a point is soon reached at which an element occurs belonging to a family already represented in the list and it is necessary to begin a new period:

Na	Mg	Al	Si	P	S	Cl
23	24	27	28	31	32	35

Soon the process must be repeated but this time the period must be extended several places.

K	Ca	Sc	Ti	V	Cr	Mn	Fe	Co Ni
39	40	44	48	51	52	55	56	59

Thus far and in the next line elements in each vertical series belong to the same family.

It is impossible to continue the arrangement on this principle. In the fifth line, a whole series of elements is encountered (the rare earth elements) all belonging to the same family: if written across the table, these do not find a place in the families occupying the space above. To get over this difficulty, I have suggested a form of classification more nearly resembling that followed in the case of the compounds of carbon and involving the recognition of series of homologous elements.

The diagram shown is that laid in 1902[1] before the Royal Society of London. The appended classified scheme of the elements is but slightly different from that brought before the Royal Society in 1902 and will be understood without further explanation. The Argonides are omitted, as they cannot be classified. Antimony and Bismuth are placed according to their atomic weights but might easily be brought into the nitrogen-arsenic column; if this change be made, five of the columns are blank and two have elements only at their head. Cobalt should be put above nickel.

The recognition of the possibility of arranging elements under families in the order of atomic weight has made it possible to foresee necessary corrections in atomic weights in cases in which the element, when properly arranged in its family, appears to be out of place in weight. Several such forecasts have been justified—there is, however, one obstinate exception, *Tellurium.* This element should come in advance of iodine, as it belongs to the oxygen-sulphur family, which is in advance of the fluorine-chlorine family. The accepted atomic weight of tellurium, however, is about a unit greater than that of iodine—so that if arranged in the order of its weight it falls after iodine and is out of place. Time after time the atomic weights of tellurium and iodine have been redetermined but without disturbing the relationship.

It appears to me that a possible explanation of the difficulty that tellurium introduces may be found in the assumption that the genetic connexion between the elements is not necessarily always through one and the same direct line—the conventional assumption. Characteristic of the method of classification I have advocated is the existence, not only in the case of the rare earth elements but in others also, of pockets of related or homologous elements—of sub-families. If such exist, there is no reason why in the cases in which only single elements are not known there should not also be several. It is conceivable, from this point of view, that tellurium belongs to a sub-family of which it is not the head and that iodine is to be regarded as descended in direct succession from the missing head of the tellurium sub-family, not from tellurium itself.

But the argonides have introduced new difficulties. As they have no chemical properties, in classifying them we have

[1] *Proceedings*, vol. lxx. p. 86.

1 H	2	3	4	5	6	7 Li	8	9 Be	10	11 B	12 C	13	14 N	15	16 O
17	18	19 F	20	21	22	23 Na	24 Mg	25	26	27 Al	28 Si	29	30	31 P	32 S
33	34	35 Cl	36	37	38	39 K	40 Ca	41	42	43 Sc	44				
											=				
											48 Ti	49	50	51 V	52 Cr
53	54	55 Mn	56 Fe												
			=												
			59 Ni / Co	60	61	62	63 Cu								
							=								
							65 Zn								
							66	67	68	69 Ga	70				
											=				
											72 Ge				
											73	74	75 As	76	77
78	79	80 Br	81												78
			82	83	84	85 Rb	86								79 Se
							87 Sr	88	89	90 Yt	91 Zr				
											92	93	94	95 Nb	96 Mo
97	98	99	100												
			=												
			102 Ru												
			103 Rh												
			=												
			105	106	107	108 Ag	109								
			=				=								
			107 Pd				112 Cd	113	114	115 In	116				
											=				
											119 Sn				
											120	121 Sb	122	123	124
125	126	127 I	128												=
			=												128 Te
			130	131	132	133 Cs	134								
							=								
							137 Ba								
							=								
							139 La								
							140 Ce								
							141 Pr								
							=								
							144 Nd								
							=								
							=								
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							150 Sm								
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							159 Tb								
							=								
							162 Dy								
							=								
							=								
							=								
							168 Er								
							169 Tu								
							170	171	172	173 Yb	174				
											=				
											=				
											=				
											180	181	182	183 Ta	184 W
185	186	187	188												
			=												
			191 Os												
			=												
			193 Ir												
			=												
			194 Pt	195	196	197 Au	198								
							=								
							200 Hg								
							201	202	203	204 Tl	205				
											=				
											207 Pb	208 Bi	209	210	211
212	213	214	215												
			=												
			220	221	222	223	224								
							=								
							226 Ra								
							=								
							228	229	230	231	232 Th				
											=				
											234	235	236	237	238 U

nothing to go upon but their atomic weights—and these we do not know. It has been proposed to group them together vertically in advance of the lithium-sodium family. But argon, assuming its atomic weight to be 40, is out of place before potassium; moreover, 40 is the atomic weight of calcium and the existence of two isometric elements having such totally different properties is without precedent: nickel and cobalt, the only parallel case, have very similar properties.

"Que faire?" My own view is that there is no evidence whatever to justify the conclusion that the argonides are true single-atom molecules and inert as elements but much to support the assumption I advocated in 1895 that the molecules of argon (and therefore those of its companions) with which we are acquainted are inert because they consist of atoms which have intense affinity for each other—of atoms so firmly locked in each other's embrace and so occupied as to be destitute of external attractive power or residual affinity. If such be the case, we cannot place them in the table at present, the table being one in which the arrangement is in accordance with atomic weights and with chemical properties—both of these are unknown in the case of the argonides.

I may here point out that in discussing the properties characteristic of elements we must be careful—as in ordinary life—not to be misled by appearances. The conception formed is obviously an abstraction based on the consideration of the behaviour of the element in its various compounds—for, in few if any cases are we in possession of the elementary material proper in its simplest or atomic form.

The influence of molecular composition on properties is altogether astounding. As we know it in the atmosphere, nitrogen—which is present in the form of molecules each composed of two atoms—is all but inert, yet we have every reason to believe that, in the atomic state, it is one of the most active of elements. Again, to take the case of carbon in the diamond—which apart from its combustibility is remarkable for its chemical inertness—we are dealing with the least coloured substance that is known to us, whilst carbon in the form of charcoal is the most highly coloured of elementary materials, indeed of known materials; the diamond is intensely hard and has no metallic properties whatever but graphitic carbon is commonly termed *black lead* on account of its resem-

blance to the metal lead. The black forms of carbon are easily oxidised by wet oxidising agents. All the forms of carbon, probably, are of highly complex molecular structure and differently constituted. But the chemist forms his opinion of carbon from the study of its compounds generally: the abstract conceptions which are associated with the term carbon in his mind have nothing in common with what is known of any of the forms of carbon, such as the diamond, graphite or charcoal. It is on this account that the study of chemistry is so difficult—because it is impossible to attach quantitative significance to so many of the chemist's conceptions.

The outlook has been changed entirely within the last few years by the discovery of radium; as the President of the British Association said last year at Portsmouth: "It has widened the bonds of thought." The discovery has put us in possession of a substance whose properties are without precedent. Radium is accounted an element, because it forms salts more or less like those of barium. But when left to itself (as salt) it breaks up spontaneously into helium and a condensible emanation, which itself is unstable and rapidly gives rise, it is asserted, to a series of products each less stable than itself, helium being a product of the decomposition at nearly every stage. It is suggested that it ultimately yields lead. But radium itself is now supposed to be derived from uranium, a substance regarded as an element before the discovery of radium; and the passage from uranium to radium is said to involve the separation of three molecules of helium from the former.

The evidence on which some of these conclusions is based is of a very special and daring character and by no means universally accepted as sufficient at present: but in such a case, at first, to meet the emergency, it is almost necessary to throw caution to the winds.

A relatively enormous amount of energy is given out from radium as the change proceeds and the helium molecules are expelled with extraordinary velocity; apparently, however, the change takes place independently of all external influences and cannot be hastened or retarded by any known means. It is in this respect particularly that the behaviour of radium is so marvellous.

Professor Turner in his address to the Mathematical and Physical Section of the British Association at Portsmouth last

year drew special attention to a paper written for students by an American geologist, Professor Chamberlin of Chicago, entitled "The Method of Multiple Working Hypotheses," in which the danger of interpreting observations in only one way is commented on.[1] A complaint that may be justly made against the workers with radium, I believe, is that they have hitherto allowed themselves to be possessed too entirely by one idea.

Too little notice, it appears to me, has been taken of the special properties of helium and the other argonides in considering the possible nature of radium. Radium *cannot* contain helium *as we know it*, any more than ammonia contains nitrogen *as we know it*. The nitrogen in ammonia is the active atomic form of nitrogen—unknown in the free state—which combines with itself to form the practically inert molecules we handle when dealing with atmospheric nitrogen gas. If we apply this argument to radium, we can only regard it as *a compound containing an active form of helium which in combination with itself*

[1] With regard to the use of the term element as applied to radium and similar substances, the tendency in modern times has been towards precision in language. It is the practice to draw a definite distinction between the terms atom and molecule and to confine the latter to the physical or kinetic units or moving masses distinguishable in a substance. Graham's life work was the study of the motions of molecules. The tendency referred to is the outcome of the general recognition of Avogadro's theorem as the one means at our disposal of determining relative molecular weights. Previously the terms atom and molecule were scarcely distinguishable. Whenever Clerk-Maxwell, for example, spoke of atoms, he usually meant molecules—the kinetic units in a gas. Graham, in his early work on salts, following the practice of his time, speaks of atoms of water. Whilst the modern chemist regards the molecule as the physical or kinetic unit, he regards the atom as the chemical unit—the unit active within the molecule of a compound—and he speaks of groups of atoms which act as combining units as compound radicles, regarding atoms as simple radicles. But the atom has long been looked upon as the undivided quantity, not as the indivisible quantity. Moreover, whilst the chemist has long been willing to admit that the properties of the elements are those of a series of genetically, more or less closely related compounds, he has always thought of them as compounds of an order very different from that to which those commonly known belong. None the less, the term element has a very restricted meaning in his mind and can scarcely be applied to a substance proved to be composite: an essential connotation of the term is undoubtedly that of a primary substance in the strictest sense—a substance that has been resolved has parted with the right to rank as an element proper. To argue otherwise would be illogical: it would be as justifiable to apply the term element to benzene or any other unsaturated compound capable of combining as a whole, as it is to speak of radium as an element now that it is shown to be compound. We need, in fact, a special name for substances such as radium.

constitutes molecules of helium such as we know. Apart from the spontaneity with which it occurs and the fact that apparently it is not subject to external influences, the decomposition of radium regarded as a helide or compound of atomic helium is no more remarkable than that of liquid ozone or of nitrogen chloride, both of which explode under slight provocation. In these cases the "source" of the energy that is liberated is clearly traceable to the process whereby, in the one case, atoms of oxygen, in the other, atoms of nitrogen and chlorine, become united to form molecules—thus:

$$2O_3 = 2O_2 + 2O\,;\ O + O = O_2$$
$$2NCl_3 = 2N + 6Cl\,;\ N + N = N_2\,;\ 3(Cl + Cl) = 3Cl_2$$

After recalling how much the march of science in early days was retarded by the tyranny of a theory formulated too hastily and how in later times attempts have been made to remedy this evil by holding the theory, provisionally only, as a working hypothesis, Professor Chamberlin points out that even the working hypothesis had serious disadvantages:

"Instinctively there is a special searching out of phenomena that support it, for the mind is led by its desires. . . . From an unduly favoured child it readily grows to be a master and leads its author whithersoever it will. . . . Unless the theory happens perchance to be the true one, all hope of the best results is gone. To be sure, truth may be brought forth by an investigator dominated by a false ruling idea. His very errors may indeed stimulate investigation on the part of others. But the condition is scarcely the less unfortunate.

. . . . . .

"To avoid this grave danger the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. . . . In developing the multiple hypotheses, the effort is to bring up into view every rational explanation of the phenomenon in hand and to develop every tenable hypothesis as to its nature, cause or origin and to give all of these as impartially as possible a working form and a due place in the investigation. The investigator thus becomes the parent of a family of hypotheses: and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted."

If helium atoms, as I suppose, are endowed with activity in an extraordinary degree, the energy developed by their union in the formation of molecules may well be very, very great.

It is commonly assumed that helium and the allied gases are inert chemically: the fact that they give brilliant spectra under the influence of the electrical discharge appears to me to be direct disproof of the correctness of this view. In making this statement I am assuming that electrical discharge in gases takes place in virtue of the formation of complex systems and is an electrolytic process.

I see no particular difficulty in postulating the existence of even a complex series of helium compounds; that such compounds would exhibit remarkable properties is to be expected.

One remark in passing—the "emanation" which is the first product of the decomposition of radium has been classed as an element of the argon group. But as it is eminently unstable, this seems to be an illogical conclusion, as superlative stability is the one property distinctive of the argonides. It is a striking fact that this "emanation," itself derived from a metal, is gaseous and non-metallic, especially as it is supposed that the ultimate product afforded by radium is the metal lead. The change, in fact, is one that is almost parallel with that noticeable in the sudden passage, in the case of the elements, from metal to non-metal—which is so remarkable.

The discovery of radium has caught the popular fancy; naturally enough the astounding behaviour of the substance has led to all sorts of expectations being formed of its potentialities and the credulity of the public has been much imposed upon in consequence; there is little doubt that its curative virtues have been grossly exaggerated and that it has been given a fictitious value; the day should be at hand when calmer reflection and experience will show that the wish has too often been father to the thought. As to the decompositions effected through the agency of radium, of which we have often heard, all the information the President of the British Association could give the world last autumn was that *experiments appeared to show* that under the influence of the energy set free from radium the metal copper was converted partially into lithium and that the elements thorium, titanium, zirconium and silicon were degraded by it into carbon; but chemists ask for more than "appearances" and can only accept definitely established facts.

So far as we know at present, the elements we have long regarded as elements for the most part remain elements and much as they were in Graham's days—yet in the interval

the probability that the elements generally are composite materials may be said to have grown apace and the views that Graham held as to the essential unity of matter have in no wise lost force

I may now summarise my conclusions, so that they may be clear to those who are not specialists. First, let me again insist that the elements which the chemist attempts to classify are not the actual materials known to the general public but certain abstract conceptions formed by considering the properties and inter-relationships of substances generally. Classification is not usually based on mere matters of fact, so called; interpretation plays a large part and artistic feeling also, as what are termed properties are mostly the expression of reciprocal relationships and not absolute possessions.[1] Every one knows that the convenience of a house stands in no very distinct relationship to the materials of which it is built but is mainly determined by its structure and dimensions. So it is in chemistry—structure is the most important factor in determining properties. Thus the great majority of compounds of carbon and hydrogen are colourless; a few of a particular structure are yellow or red; and by combining oxygen or nitrogen in certain ways with carbon and hydrogen, so that a certain definite character of structure is developed, we can obtain every shade of colour we desire.

Taken generally, the elements present properties which are those of a set of more or less closely related compounds; indeed, it is possible to arrange carbon compounds in order of molecular weight in series and groups so as to match very closely, within certain limits, the alterations in properties which are noticeable in passing through the series of elements. The unequal development in various parts of the system is common both to the elements and to the carbon compounds —in the case of these latter, compounds which "theoretically" might be expected to exist have not infrequently proved to be unobtainable. We may apply this argument to account for some, at least, of the gaps in the record of elements.

The one difference among elements which cannot well be

[1] The present-day tendency to reduce everything to numerical terms and to exaggerate the value of decimals is but the expression of the fact that there are many artificers but few artists.

matched in the case of carbon compounds is that noticeable, for example, between the elements oxygen and tellurium, the one being entirely non-metallic in its behaviour, whilst the latter simulates a metal and is therefore often classed as a metalloid. But these two elements are clearly members of a family and it is to be supposed that they are genetically related; if so, there must be some factor which conditions metallicity and this factor apparently tends to come into operation more and more as the "atomic weight" increases in the case of the elements.

It appears to me that a legitimate provisional explanation of the wonders of radium is to be found in the as yet unrecognisable wondrous properties of helium—not of helium as we know it but of atomic helium or helium-stuff.

The hypothesis which I ventured to suggest when the discovery of argon was brought under the notice of the Royal Society, in 1895, has gained in probability since the discovery has been made that the allied element helium is a product of the degradation of radium. If, as I not only think possible but now believe to be the case, the atoms of helium and of the allied inert gases are gifted with intense activity, far beyond anything we know of, it may well be that when such atoms enter into combination, either with one another or with other elements, the amount of energy set free is very great[1] and that when they combine with other materials they produce changes in properties very different from and far more profound than those we know of at present.

That my hypothesis is not an impossible one even physicists will admit, I think; at all events, I may claim the support of Professor Rutherford, as expressed in a paragraph in the address he delivered in Winnipeg about two years ago:

"Since in a large number of cases the transformations of the atoms are accompanied by the expulsion of one or more charged atoms of helium, it is difficult to avoid the con-

[1] Sir George Darwin, in the recent edition of his book on *The Tides*, speaking of radio-activity, remarks: "We have recently learned that atoms may in themselves possess a colossal store of energy which is quite distinct from that of chemical affinity." It is clear from such a statement how little the position is understood. There is nothing to show that the energy developed by the decomposition of radium is not that of chemical affinity.

clusion that the atoms of the radio-active elements are built up, in part at least, of helium atoms. It is certainly very remarkable and may prove of great significance that helium, which is regarded from the ordinary chemical standpoint as an inert element, plays such an important part in the constitution of the atoms of uranium, thorium and radium."

Moreover, while writing this account, I have been surprised to find a very remarkable communication by the late Professor Johnstone Stoney (in his day one of the highest authorities on questions of molecular physics) published in the *Chemical News*, in 1895,[1] the week after a full account was first given of argon. Professor Johnstone Stoney suggested that the properties of argon are compatible with the view that this "element" is a compound of the type of a paraffin hydrocarbon, such as octane, derived from one of the unknown elements falling between hydrogen and lithium.

I therefore do not stand quite alone as a heretic and should find some companions at the stake.

I will add, in conclusion, that I have advisedly abstained from discussing the sub-atomic, electrical (electronic) hypothesis of the constitution of matter which is now under consideration. Frankly, I am not yet satisfied that this stands on a valid basis of ascertained fact, my opinion being that the problem of electrical conductivity in gases has not been studied with all the precautions and the thoroughness that is necessary to justify us in accepting the far-reaching conclusions physicists have based upon their observations.[2] The occasion is one on which it is incumbent on us to use a variety of hypotheses.

[1] Vol. lxxi. p. 67.

[2] I have no wish to minimise the value of the observations and of the arguments that are based upon them. But not only is it easy to be possessed by one idea—there is also the difficulty, we must all admit, in which we are placed, that of understanding one another. At the present day, in all discussion of the intricate problems to which workers are devoting their attention in the various fields of scientific inquiry, the difficulty arises that we do not as yet—and perhaps cannot—see eye to eye; there is no doubt that various types of mind are required to appreciate the various points of view from which the problems can be considered and that it is rare to find in one mind powers of appreciation sufficiently varied to comprehend all the possible issues. In the course of my career I have only met one man who has appeared to me to be capable of appreciating the several points of view of different types of workers and of acting as arbitrator in cases such as that under consideration—the late Professor Fitzgerald of Dublin. He could

2. The Diffusion of Liquids

I pass to Graham's work on the diffusion of liquids—that is to say of substances dissolved in water. To discuss this properly would involve a complete discussion of the process of dissolution and of the problems afforded by solutions generally.

The words with which he opens his first account of his work on the diffusion of liquids in 1849 show that little was known when he began to study the subject:

"Any saline or other soluble substance once liquefied and in a state of solution is evidently spread or diffused uniformly through the mass of the solvent by a spontaneous process.

"It has often been asked whether this process is of the nature of the diffusion of gases but no satisfactory answer to the question appears to be obtained. . . ."

He evidently expected to find some kind of simple molecular relationship, as he remarks:

"A diffusibility like that of gases, if it exists in liquids, should afford means for the separation and decomposition even of unequally diffusible substances and being of a purely physical character, the necessary consequence and index of *density*, should present a scale of densities for substances in the state of solution analogous to vapour densities, which would be new to molecular theory."

But it is clear that he was aware that a variety of considerations should be taken into account, especially the relationship between solubility and diffusibility; this is shown in the following passages:

"In the consideration of solubility, attention is generally engrossed entirely by the quantity of salt dissolved. But it is necessary to apprehend clearly another character of solution, namely the degree of force with which salt is held in solution or

sympathise with both sides—with chemist and physicist and, I think, with the biologist also—and could give mathematical form to their ideas without losing sense of proportion. A balanced mind such as his seems to be almost impossible in these days—the burden of knowledge is too great. I make these remarks, as I feel that the fast-spreading disease of specialisation is one that we need to watch most carefully and guard against. No form of cruelty probably is worse or more fatal in its effects than that of "over-educating" genius—perhaps if we did more in our schools in the way of providing opportunity for free mental development, less in the way of forcing lesson-learning, the results would be more satisfactory.

the intensity of the solvent attraction, quite irrespective of quantity dissolved. . . .

"Besides being said to be small or great, the solubility of a substance has also therefore to be described as weak or strong.

"The gradations of intensity observed in the solvent force are particularly referred to, because the inquiry may arise how far these gradations are dependent upon unequal diffusibility; whether indeed rapidity of diffusion is not a measure of the force in question."

The most important conclusions at which he ultimately arrived was that liquid diffusion supplies the densities of a new kind of molecule but nothing more respecting them. And on this he remarks:

"The fact that the relations in diffusion of different substances refer to equal weights of those substances and not to their atomic weight or equivalents is one which reaches to the very basis of molecular chemistry. The relation most frequently possessed is that of equality, the relation of all others most easily observed. In liquid diffusion we appear to deal no longer with chemical equivalents or the Daltonian atoms but with masses even more simply related to each other in weight. Founding still upon the chemical atoms, we may suppose that they can group together in such numbers as to form new and larger molecules of equal weight for different substances or if not of equal weight of weights which appear to have a simple relation to each other. It is this new class of molecules which appears to play a part in solubility and liquid diffusion and not the atoms of chemical combination."

The question we may now ask is—What is the class of molecules which are concerned in solubility and liquid diffusion?

It may appear surprising that, although engaged in following the motions of molecules, Graham apparently neither worked nor thought in terms of molecular magnitude—that he always used solutions of this or that percentage strength. But in his day, the minds of chemists were not possessed by Avogadro's theorem—nor are they even now, perhaps, in any proper way.

Unfortunately his comparative experiments were made with solutions containing equal weights of the substances diffused; but as he found that the diffusibility of a salt was practically proportional to the amount present, it follows from his observations that of two salts such as sodium and potassium chloride,

which are equi-diffusive when present in equal weights, that of greater molecular weight is the more diffusible, as the salts should be dissolved in the ratio of 74·5 to 58·5 not that of equal weights to be present in equal molecular proportions. Similarly, sodium chloride and nitrate, instead of being about equi-diffusive, as Graham thought, differ considerably, as the diffusivity of 85 parts of the nitrate should be contrasted with that of 58·5 of chloride. Sodium and magnesium sulphate also, from this point of view, are more diffusive than sodium chloride.

It must be confessed that much of the work on diffusion has not been carried out in a way that is calculated to give the information required for the comparison of the behaviour of substances under comparable conditions. As the water plays its part in the process as well as the dissolved substance and must also be thought of as diffusing, it should be used in some constant molecular proportion, if we are to ascertain the behaviour of a variety of substances relatively to water. No measurements have been made hitherto which satisfy this condition. The nearest approach are those of Long, who determined the diffusivity of a considerable number of salts from solutions containing one equivalent of the salt per litre. The results quoted in the following table show the relative number of molecules of the different salts that diffuse under equal conditions, according to Long's determinations; it is to be noted, however, that the observations are not always in agreement with those made by Graham:

RELATIVE MOLECULAR DIFFUSIVITIES OF SALTS

	Cl	Br	I	NO_3	SO_4
K	803	811	823	607	—
Na	600	509	680	512	678
NH_4	689	629	672	524	724
Li	541	—	—	512	—
Ba	450	—	—	656	—
Sr	430	—	—	552	—
Ca	429	—	—	—	—
Mg	392	—	—	—	348

These values are significant as showing that liquid diffusion, unlike gaseous diffusion, is not a process in which the molecular weights of the dissolved substance are determinative of the rate of translation of the molecules. But a clue to the

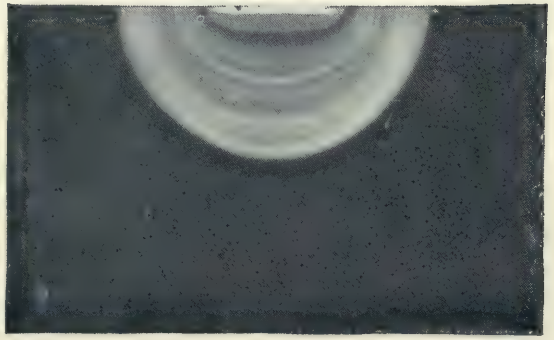


FIG. 1.

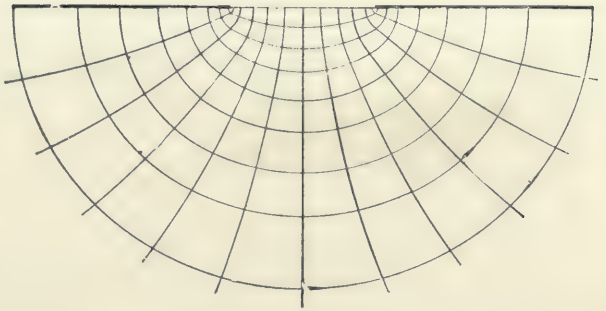


FIG. 1*a*.

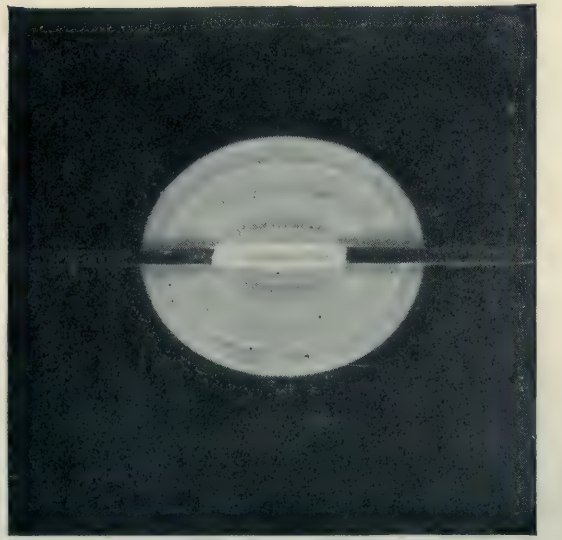


FIG. 2.

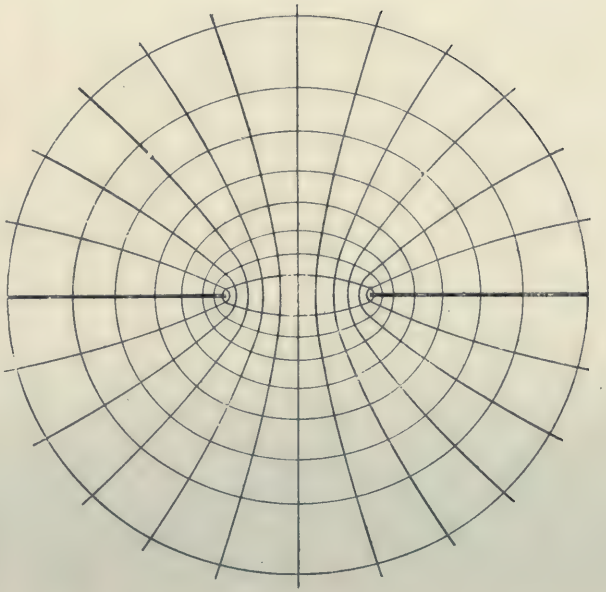


FIG. 2*a*.

differences is probably to be found in the fact that, taking comparable salts into account such as potassium, sodium, ammonium and lithium chlorides, the less diffusive compounds are the more highly hydrated. Speaking generally, the least hydrated salts appear to be the most diffusive.

But on the other hand, according to Graham, such substances as hydrogen cyanide, ammonia and acetic acid are less diffusive than hydrogen chloride; alcohol is less diffusive than acetic acid and sulphurous acid is less diffusive than sulphuric. The subject is full of riddles unsolved.

Most interesting information of the manner in which diffusion proceeds has been furnished by experiments carried out by Dr. H. T. Brown in which two substances capable of interacting and forming a precipitate, such as barium chloride and sodium sulphate, were allowed to diffuse into one another through apertures of various shapes, the one substance being contained in a solid jelly, so that the form of the field of diffusion became revealed by the deposition of the precipitate within the jelly. When diffusion takes place through a circular aperture, the precipitate takes the form of a spheroidal mass resembling in shape the head of an inverted mushroom. By arranging that the diffusion of the sulphate is intermittent or by alternating the diffusion of a sulphate with that of a chromate, well-marked zonings are produced in the diffusion spheroid which indicate the zones of equal density of the diffusing substances.

Dr. Brown has arrived at the striking conclusion that the surfaces of equal density of a diffusing substance are exactly analogous to the equipotential surfaces generated in an electric field and that the stream lines of the diffusing substances correspond to the lines or tubes of force in the electric field. That this is the case is clearly shown in the accompanying figures, for which I am indebted to Dr. Brown, taken from his discourse published in the Proceedings of the Royal Institution, 1901.

Figs. 1, 2, 3, 4 are from photographs of the diffusion spheroids produced in gelatin in the manner described: 1, through a semicircular aperture the straight edge of which was close up to the side of the vessel containing the jelly; 2, through a slot in a plate covering the jelly; 3, through two contiguous apertures, the diameter of which was in the ratio of two to one; and 4, from between two parallel plates.

Figs. 1*a*, 2*a*, 3*a*, 4*a* show the distribution of the surfaces of equal electric potential in the neighbourhood of electrically charged bodies in cases corresponding to the cases of diffusion that are represented.

The regular distribution of the diffusing substance is clearly established by these observations. It is not so easy apparently to ascertain what diffuses and how diffusion takes place.

The forces at work within a solution must be such as to give rise to very complicated conditions and we are only beginning to take these into account. The fact that water itself is not a simple substance has not yet met with general recognition; the disturbances that are effected in the water when substances are dissolved in it have scarcely been considered. Of late years, in science, as in ordinary life, fashion has ruled the day and there has been a tendency to adopt extremes. "Authority," exercised through text-books and fostered by our examination system, dictates the fashion. Students of chemistry all the world over have been led to profess their belief in the doctrine of electrolytic, ionic dissociation, much for the same reason that they have turned up their trousers, not because the practice is rational but because it is conventional; so it comes that attention has been concentrated almost entirely on the dissolved substance. But the electrolytic dissociation hypothesis does not help us in the least in dealing with diffusion.

There has been moreover an unfortunate tendency to regard liquids from the point of view of gases and to suppose that what is true of one is true of the other; the analogies the two states present have—improperly, I contend—been regarded as similarities.

If a closed porous vessel provided with a gauge be enclosed in a vessel containing air at atmospheric pressure, the gauge indicates no pressure within the vessel—not because there is no pressure within it but because the pressures without and within are in equilibrium; because the air particles pass into and from the vessel with equal rapidity. If hydrogen be introduced into the outer atmosphere, the gauge rises, showing an increase of pressure within the porous vessel. Graham has taught us that hydrogen molecules move four times as rapidly as oxygen molecules and it is because hydrogen molecules get in so much faster than air particles get out that the pressure rises at first; but it falls gradually again to zero, because air particles continue to pass out into the hydrogen; ultimately the mixture has the same composition within and without the porous vessel. If the vessel be of such material that only one of the gases can pass through its walls, the pressure to which the

FIG. 3.

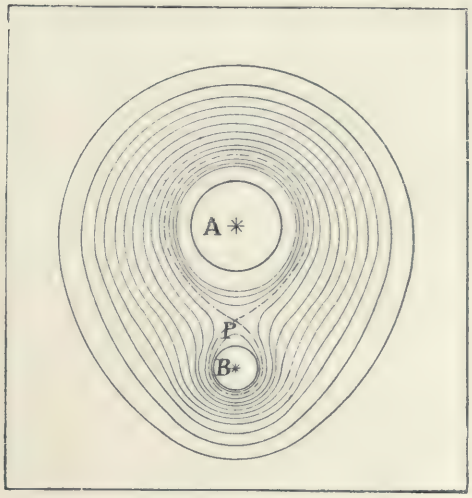


FIG. 3*a*.

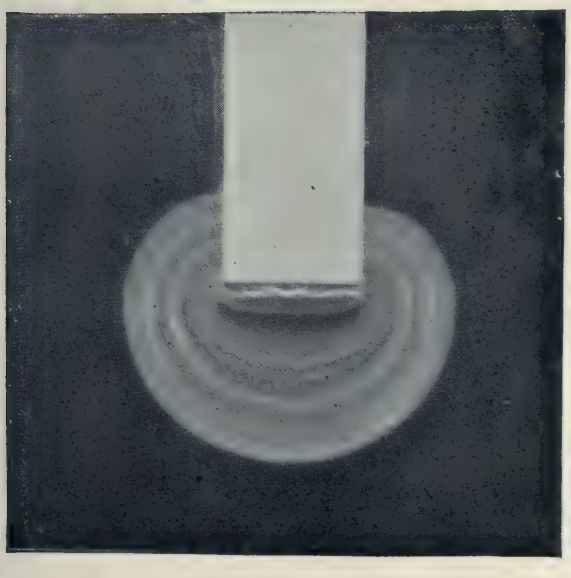


FIG. 4.

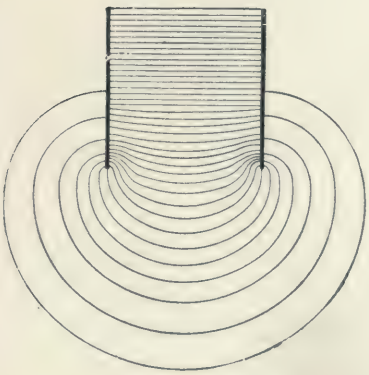


FIG. 4*a*.

entering gas gives rise within the vessel may be maintained. Thus if a heated palladium vessel containing nitrogen, at a pressure say of 200 millimetres, be surrounded with hydrogen at atmospheric pressure, the hydrogen enters but no nitrogen passes out; ultimately, when the vessel is full of hydrogen, in the sense that the molecules of this gas pass out as fast as they pass in, the pressure within the vessel rises to that of the hydrogen without plus that originally exerted by the nitrogen; the excess of pressure within the vessel is obviously that produced by the impact of the nitrogen molecules beyond that produced by the impact of the hydrogen molecules. The palladium therefore acts as a differential septum in separating hydrogen from nitrogen.

In like manner, differential septa may be prepared through which water will pass but not dissolved substances such as sugar and salt. If a closed vessel impermeable to sugar, provided with a fine gauge and filled with a solution of sugar, be placed in water, the pressure gradually rises to a maximum corresponding to the strength of the solution, the rise being due to the passage of water inwards. For reasons which are in no way clear to us at present, in dilute solutions the pressure developed in the presence of substances such as sugar is the same as the pressure the substance would exert if present in the gaseous state in the volume occupied by the solution.

The pressure shown by the gauge is unfortunately spoken of as the osmotic pressure of the solution and has been attributed to the dissolved substance directly, as though it were acting as a gas. But the two cases are very different. In the one, there is room for the entry of the gas and all that happens is that the pressure rises within the vessel; moreover, the gas which cannot pass out exercises no attractive power upon the entering gas. In the case of the solution, as liquids are but very slightly compressible, there is practically no room for the entering water and expansion takes place into the gauge. There can be no doubt that osmotic pressure, so-called, is not a pressure in the ordinary sense but a negative pressure or attraction; solutions *in water*, in fact, are all more or less *attractive of water*.

When diffusion takes place into water from a solution in water, obviously as the substance diffuses out from the solution water must diffuse into the solution; diffusion must be a process of exchange. It is conceivable that the attraction which solutions

have for water is the cause of diffusion—that the diffusing substance is pushed out from the solution by the entry of water. One of the few passages in Graham's account of his experiments in which he departs from his practice of merely recording results and makes it clear that his mind was at work endeavouring to unravel the process of diffusion has a bearing on this point. After dwelling on the retardation of the diffusion of chloride of copper by the presence of a large excess of hydrogen chloride in solution and on the similar retardation of the diffusion of common salt by hydrogen chloride, he remarks :

" These remarks are interesting in a very different point of view. I have always watched for the appearance of some absorbent or imbibing power on the part of the acids more analogous to an endosmotic attraction for water as usually conceived. If such an attraction existed, it would complicate the phenomena of diffusion, for the volume of water absorbed by the acid would displace and project a portion of the latter into the reservoir, the phial not being extensible. The high diffusibility of hydrochloric and nitric acids would be thus explained. But by such a mechanical displacement the chloride of sodium would be thrown out in the preceding experiment, as well as the hydrochloric acid, which is not the case."

It would seem probable therefore that diffusion is not a simple displacement effect but a process of direct interchange of water and other molecules and the outcome of some attractive influence reciprocally exerted by the dissolved molecules of the substance and those of the surrounding water—not a mere kinetic phenomenon but one that has to be considered also, if not solely, from the point of view of the chemist.

The fact that very soluble substances which are highly attractive of water, such as the inorganic acids and bases, are the most diffusive is in accordance with this view.

It is well known that very finely divided particles suspended in a liquid exhibit so-called Brownian movements. The character of the movement, it has been said, irresistibly impresses the observer with the idea that the particles are hurled hither and thither by the action of forces resident in the solution and that these can only arise from the continuous movement of the invisible molecules of which the fluid is composed. The invisible molecules of a divided substance must equally be thought of as similarly exposed to forces resident within the solution—but it may be questioned whether, in the case both of the Brownian

movement and that of invisible dissolved molecules, besides simple kinetic forces there are not also oscillatory forces at work which have their origin in chemical changes within the liquid.

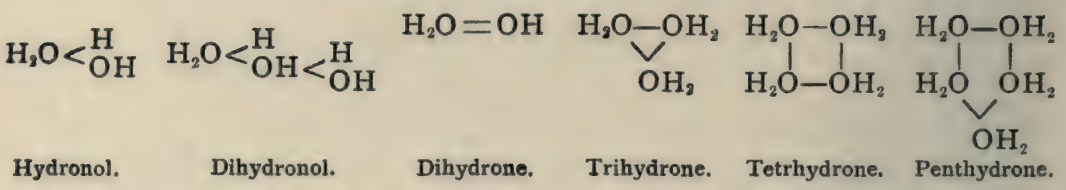
It is impossible to think of the dissolution of a solid substance by water as a merely mechanical process in which a solid mass is resolved into its constituent molecules and these are distributed among the water molecules. Dissolution, as Graham indeed implies, must be regarded as conditioned by the existence of a definite affinity between the dissolved substance and the solution and as maintained, when once effected, in virtue of the continued operation of such affinity.

Unfortunately, having once learnt to regard water as a compound of the two elements hydrogen and oxygen and to represent its molecular composition by the simple formula H_2O, chemists have insensibly come to regard water as a simple substance. The habit of speaking of *water* as existing in three different *physical* states has so grown upon us, that we have failed to recognise that it is different physically in its three states because these differ chemically. If a chemical change be defined as any alteration in molecular composition or configuration, most cases of so-called physical change, in my opinion, are cases of chemical change. I submit that we are as much obliged to regard the passage of water from solid to liquid or from liquid to gas as a case of chemical change as we are that of red to yellow phosphorus — to suppose, that is to say, that the changes of state are either depolymerisation or polymerisation phenomena.

I have proposed the name *Hydrone* for water in its simplest molecular form—that of steam, to which alone the formula H_2O is applicable. Liquid water may be supposed to be a mixture of various " polyhydrones " with more or less hydrone, the amount of this latter being greater the higher the temperature.[1]

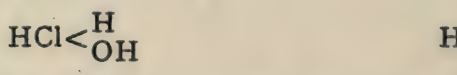
We have no means of determining the composition of the polyhydrones present in water and ice; we can only consider possibilities and probabilities. The argument from analogy justifies us in believing that a series of complex molecules may exist. On the present occasion I will merely call attention to the following formulæ representing compounds of the hydrone molecule which are conceivably present in water :

[1] The subject is more fully discussed in articles published in SCIENCE PROGRESS in January and April 1909.



Hydronol. Dihydronol. Dihydrone. Trihydrone. Tetrhydrone. Penthydrone.

When a substance such as hydrogen chloride is dissolved, it has no cause to undergo molecular simplification but it may be hydrolated and hydronated in various ways and there is reason to suppose also that it may not only "divide" hydrone but may be "divided" by it, thus:



A compound such as common salt would behave in a similar manner and perhaps give rise to several compounds with hydrone but being a polymerised substance, such as is represented by the formula $(NaCl)_x$, it would not be completely resolved into the simple molecules represented by the symbol NaCl except in very dilute solutions.

We have consequently to consider the diffusion not of a single substance but of an unknown number of substances and the problem becomes one of great complexity. It is therefore scarcely probable that we shall ever arrive at any very absolute knowledge of all the separate factors concerned in diffusion. The great lesson we are now learning is, indeed, that the problems are of such complexity that there is little probability that we shall be able to unravel more than the broad outlines of the processes at work in solutions.

The greatest service rendered to science by Graham in his studies of liquid diffusion is undoubtedly that arising from the distinction he drew between the colloid or jelly-like and the crystalline states of matter and his introduction of colloid membranes or septa as a means of separating the two kinds of substance by what he called *dialysis*, thus giving the clue to the interpretation of a large class of vital phenomena. No better illustration of the difference can be given than that contained in the following passage from his communication to the Royal Society on Liquid Diffusion applied to Analysis:

"A sheet of very thin and well-sized letter paper, of French manufacture, having no porosity, was first thoroughly wetted and then laid upon the surface of water contained in a small

basin of less diameter than the width of the paper and the latter depressed in the centre so as to form a tray or cavity capable of holding a liquid. The liquid placed upon the paper was a mixed solution of cane sugar and gum arabic containing 5 per cent. of each substance. The pure water below and the mixed solution above were therefore separated only by the thickness of the wet sized paper. After twenty-four hours the upper liquid appeared to have increased sensibly in volume, through the agency of osmose. The water below was found now to contain three-fourths of the whole sugar, in a condition so pure as to crystallise when the liquid was evaporated on a water bath."

The sized paper, Graham points out, has no power to act as a filter. Molecules only, he says, permeate the septum—not masses. But diffusion takes place through the water contained in interspaces in the colloid. He introduced the use of vegetable parchment as a dialysing medium.

Colloids are substances which diffuse with extreme slowness. Their presence in water opposes little if any hindrance to the diffusion of crystalloids, diffusion taking place as easily from a solid jelly containing several per cent. of gelatin as from a simple aqueous solution.

Colloid septa are of special interest as the lining membranes of vegetable and animal cells and living tissues generally are of this nature. But the natural septa are far more perfect than Graham's vegetable parchment—they are not permeable by salts and many other crystalloids which pass through such a septum. No better illustration can be given of the part such septa play than that afforded by the barley grain. Obviously, it is important that the food materials stored up in seeds should not pass outwards during germination and also that harmful materials should not pass in and check or stop growth. Prof. Adrian Brown has shown recently [1] that cereal grains are provided with a lining membrane close to the outer skin of the seed which has most remarkable power as a differential septum. He has demonstrated this in a most beautiful way by taking advantage of the fact that in certain blue barleys the layer referred to contains a colouring matter, resembling litmus, which is reddened by acids. When the barley grain is steeped in water it gradually takes up water to the extent of 70 to 80 per cent. of its weight. It equally absorbs water from dilute solutions of strong acids such as sulphuric acid but may be kept in the acid liquid during

[1] Proceedings of the Royal Society, B. vol. lxxxi. p. 82 (1909).

many days without the colour changing; in like manner, strong alkalies such as potash and soda, cannot gain an entry into the seed from their solutions, provided these are not so strong as to exercise a solvent action on the membrane. And not only are acids, alkalies and most salts kept out but sugars also and the other substances which are formed during germination and serve as food for the growing plant. Strange to say, however, the weak organic acids, carbonic acid, ammonia and a large number of neutral organic substances (hydrocarbons, alcohols, ethers, chloroform, iodine and a few salts such as chloride of mercury or corrosive sublimate) readily pass into the grain from aqueous solutions.

Experiments which my son and I have carried out with leaves[1] show that these are provided with differential septa which protect them in precisely the same way that the cereal grain is protected. We have proposed to apply the term *Hormone* collectively to the class of substances capable of penetrating such septa, because of the stimulative effect which they produce when they gain an entry.

This is shown in the case of the common laurel by the liberation of hydrogen cyanide, which is easily detected by the change of colour it produces, from yellow to brown, when paper dipped in an alkaline solution of picric acid is exposed to its action. A still simpler demonstration may be given by means of the spotted Japanese laurel, *Aucuba japonica*. When exposed to the action of Hormones, this blackens more or less rapidly according to the rate at which the hormone passes into the leaf.

Such leaves contain certain complex compounds known as glucosides together with corresponding enzymes or ferments which can act upon the glucosides and resolve them into simpler compounds; under normal conditions the glucoside and enzyme are kept apart apparently but the effect of the hormone is to bring them together in some way that we do not at present altogether understand, though it is probably in large measure due to the fact that the entry of the hormone into the cell is necessarily followed by the diffusion of water, so that the cell sap is diluted, a condition which favours the action of the enzyme.

It is more than probable, I think, that hormones are in large measure the regulators of the vital mechanism. Mankind, all the world over, indulges in stimulants, unconsciously if not con-

[1] *Ibid.* B. vol. lxxxii. p. 588 (1910): *Annals of Botany*, vol. xxv. p. 507 (1911).

sciously—even when he wears a blue riband—and stimulants of some kind seem to be necessary to life. Apparently the germination of seeds is not a spontaneous process dependent only on temperature and the presence of sufficient water—but is initiated by the very minute quantities of ammonia and also by the carbonic acid present in the soil. Recent observations have shown that soil which has been heated moderately by steam is far more fertile than unheated soil. Normally soil is full of organisms, protozoa and bacteria ; the former live at the expense of the latter but the latter manufacture ammonia and carbonic acid out of the organic matter in the soil, so that if their numbers are diminished, these hormones are formed in diminished amount. Heating kills off both protozoa and bacteria but the spores of the latter survive and a fresh race soon comes into existence and the soil becomes richer in them than it was when the protozoa were present. The increased production of ammonia and carbonic acid thus entailed has the effect of stimulating plants grown in the soil to increased growth.

Probably if we ate food free from hormones it would be indigestible—the hormones serve to liberate the enzymes which determine the resolution of our food into simpler soluble materials which can enter the circulation and be conveyed all over the body. Not only is alcohol (in strict moderation) a digestive stimulant but carbonic acid is also, so that those who drink soda water are open to the reproach—if it be one—that they take stimulants. The onion which the farm labourer eats with his bread and cheese not only gives flavour to his meal but also promotes its digestion—as the onion contains a glucoside and a ferment which interact when brought together, giving rise to oil of garlic—the cause of the characteristic onion smell—which is an extraordinarily powerful stimulant. When these things are better understood, there will be less nonsense talked about stimulants—it will then be realised that we are all equally concerned in their use and that what we have to avoid is their use in excess, though it may be difficult for us to determine what constitutes excess. In any case, we have learnt much during the past few years that enables us to understand the manner in which alcohol and other stimulants act and to regard their use from a rational point of view instead of those of prejudice and sentiment. Graham's work affords the basis from which such problems must be considered.

Finally, let me call attention to the striking illustration of diffusive activity through colloid membranes and at the same time of the stimulating effects produced by hormones that is afforded by the observations made by Dr. Jacques Loeb, who has succeeded in causing unfertilised sea-urchins' eggs to develop into swimming larvæ by placing them during a minute or so in sea water containing a minute proportion of butyric acid and then in sea water deprived of oxygen, afterwards restoring them to ordinary sea water. A number of other substances have a like effect.

It is in directions such as these that inquiry is tending at the present day—as it is apparent to an extent never obvious before that complete understanding of the behaviour of colloids is an essential preliminary to an appreciation of the chemical changes which constitute life.

I trust that I have been successful in indicating some of the ways in which the foundations well and truly laid by Graham are being utilised by modern builders. The scientific caution which he displayed, his reticence in explaining his results, are more than justified by the fact that at the present day we know little more than he knew as to the precise nature of the process of diffusion—though undoubtedly we begin to see our way towards the light.

We need another Graham to take up the subject afresh and give us the multitude of facts that are required to make even an approximate interpretation of the process possible.

H. E. A.

MODERN USES OF THE METAL ALUMINIUM

By RICHARD SELIGMAN, Ph.D.

Aluminium, which is the chief component of all clays and an important constituent of many rocks, is one of the most widely distributed chemical elements. Despite this fact, it was not isolated until the year 1827, when Wöhler obtained the metal in the form of minute grey scales by the interaction of aluminium chloride and metallic potassium. Although this method was improved upon by St. Clair Deville, aluminium did not become a common metal until the simultaneous discoveries of Héroult and Hall in 1887-8 permitted of its manufacture by electrolysis. The process perfected by these two inventors, which is the only one in use to-day, consists in electrolysing a solution of alumina in the molten double fluoride of aluminium and sodium known as cryolite. The electrodes used are made of carbon and the products of electrolysis are aluminium on the one hand and oxygen and the oxides of carbon on the other. The electrolysis is carried on at a temperature of 950–1000° C., so that the metal, which melts at 657° C., is obtained in the molten form.

For close on ten years after these discoveries, aluminium was still regarded as little more than a scientific curiosity but more recently it has found its way into a rapidly increasing number of industries, for many of which it has become an essential.

The rapid development of the aluminium industry is an exemplification of the rule which, though universal, is frequently unrecognised—that supply creates demand. To show that the advance is in this case governed by this rule, it will be necessary to consider the uses to which the metal has been put during the last seven years. At the beginning of the period (1904–5), owing to the perfection of electrical methods of manufacture, aluminium had been available in large quantities for close on fifteen years but the world's output was only about 9,000 tons and the price £107 per ton; the amount produced was undoubtedly in excess

of the consumption by no small amount and the makers held considerable stocks of the metal. At that time, the chief difficulty confronting the manufacturer was that of marketing his wares and in view of the hopes which had attended the inception of the industry the outlook was sufficiently discouraging. However, the time at which the aluminium industry was at its lowest ebb coincided very closely with the first strong impulse given to the automobile trade which was destined to carry it into the forefront of industrial undertakings. In the early days of self-propelled road vehicles, as at a more recent date in the case of aerial vehicles, every effort was made to lighten the burden placed upon the weak engines which did duty as tractors and in accomplishing this advantage was taken freely of the most salient feature of aluminium, its extraordinary lightness. Wherever possible, aluminium was used whether for engine parts or for the coach work. In a very short time the aluminium makers, who a few months before had been piling stock on stock, not only found their accumulations absorbed but their factories incapable of keeping pace with the rapidly growing demand. The writer can recall days as recent as 1906 when anxious hours were spent waiting for small consignments of a ton or two of metal from the reduction works to keep the rolling mills going and when every corner and cranny was searched for bits of old scrap which could be remelted to feed the apparently insatiable motor trade.

Steps were at once taken to increase the capacity of the reduction works and the extension of old and the installation of new plant became the order of the day in two continents. But the aluminium maker is wedded, for better or for worse (probably the latter), to water power and as water power takes anything from eighteen months to four years to install, the automobile maker could not wait. Faced by the imperative necessity of finding a substitute for aluminium, wherever the latter could be dispensed with, he turned to thin steel sheets, which he found not only far cheaper but also to his surprise not markedly heavier. He had overlooked the fact that weight for weight steel is stronger than aluminium, so that for many purposes he was able to reduce the thickness of the metal used to such an extent that no material increase in weight resulted. Moreover, as engine power and efficiency were increased, gradually dead weight began to be of less importance, a process

which we can see going on to-day in the development of aeroplanes. By the time then that the cumbrous water wheels, which had been installed all over Europe and America, had been made to revolve, the motor-car had swept on its course and the aluminium maker was left with his enormously increased output but robbed of the outlet for which the output had been called into being.

Thus the supply was created. By 1910–11 the world's output had been raised to 34,000 tons and as the power available is now very great and many hydraulic installations which serve other processes would be available, in case of need, for the production of aluminium, the price is half what it was at the opening of the period under review.

Now as to the demand. Faced by a surplus of metal for which there was no outlet, the manufacturers set themselves to ascertain the fields in which aluminium might best find an application. As a consequence of systematic efforts to educate potential consumers, results have been attained which a few years ago seemed beyond the dreams of avarice. In different countries different lines of action have been pursued. Thus in America the chief new applications found have been in culinary ware and the electrical industry; in Germany also the cooking utensil trade has reached enormous proportions, whilst a most promising outlet has been opened up in chemical apparatus; in France the motor trade still takes a very large amount of aluminium but a great deal of the metal produced in France finds its way into Germany to feed the industry there, no aluminium being made in Germany, which has to import all its raw metal from other countries. England, characteristically, was long content to send the metal made here abroad rather than go to the trouble either of creating new industries at home or of devoting energy to the studies necessary to enable her to do so. During the last two years, however, a great deal of spade work has been done and foundations have been laid upon which promising businesses in electrical and chemical apparatus are being built. Moreover, the motor trade, encouraged by low prices, is once more using the metal in large quantities.

In this article it is proposed to discuss the advantages and disadvantages which aluminium has for these purposes and to explain, as far as possible, the causes which have favoured its introduction into each branch of industry.

Electrical Industry

Owing to its relatively high electrical conductivity, the metal aluminium is now playing an important and steadily growing part in the distribution of electrical power. Taking the conductance of a copper cable of unit cross section as 100, aluminium of the requisite purity has a conductance of 60, the exact figure depending, as in the case of copper, upon the purity of the metal and its physical state. To carry a given amount of current, it is therefore necesary to take a bigger cable if aluminium be used, the cross section required being 1·66 that of copper. At first sight this does not seem promising but when it is remembered that the densities of aluminium and copper are 2·71 and 8·95 respectively, it will be seen that the *weights* of cable required to carry the same amount of current will be $1{\cdot}66 \times 2{\cdot}71 = 4{\cdot}49$ in the case of aluminium and $1 \times 8{\cdot}95 = 8{\cdot}95$ in that of copper. In other words, half the weight of aluminium will be required and as the cost depends upon the weight and aluminium wire is little more expensive than copper wire per ton, a very large saving in capital outlay is effected by the use of aluminium instead of copper. In the case of bare, overhead conductors, such as are largely used in young countries to convey electrical energy, the full benefit of this economy is felt and there are in addition one or two subsidiary advantages such as the decreased cost of carriage to the point where the power line is to be erected, usually in remote parts to which the cost of carriage is heavy. On the other hand, the strength of aluminium is only half that of copper but as the area of the aluminium is 1·66 times that of the copper line, the strength of the former is $0{\cdot}5 \times 1{\cdot}66 = 0{\cdot}83$ of that of the former. In consequence the sag between two poles or towers is greater where aluminium is used and the poles have therefore to be somewhat higher. The general conclusion to be drawn from these various considerations is very favourable to aluminium at the prices ruling to-day for the transmission of power by means of bare conductors.

Aluminium has so far not been found advantageous in cases in which small bare single wires are used, such as telephone and telegraph circuits. The explanation is to be found in chemical and mechanical rather than in electrical considerations. Aluminium, when exposed to the atmosphere, undergoes superficial oxida-

tion but this ceases at a certain distance from the surface, the coating formed acting protectively. In the case of large conductors, corrosion does not proceed far enough to cause any trouble, whereas the strength of a small wire may be seriously impaired or the wire may even be corroded throughout its thickness. On the other hand, by using aluminium for large switchboard connections and for "bus bars" for internal transmission of heavy currents in power stations, etc., very considerable economy may be effected. The same advantage does not accrue from the use of aluminium for insulated and armoured cables. Owing to the increased diameter of the conductor the amount of the dielectric or of the armouring has to be increased largely and the additional cost of the latter frequently more than neutralises the saving made on the cost of the metal. At existing prices, there seems to be a marked saving in the case of single-core cables of large diameter which are not heavily insulated, whereas with three-core cables or insulated cables for high tension currents, the advantage still rests with copper. Every case, therefore, requires separate consideration. A disadvantage attending the use of aluminium for all the purposes cited above is the difficulty of making joints, a difficulty which we shall see later has played so large a part in retarding the introduction of aluminium for chemical plant and one which is not to be met in the way found effective in the latter case. For electrical purposes, joints in aluminium conductors are usually made by purely mechanical means.

Aluminium is said to have been used successfully for battery connections in storage battery installations but the fact that in such a case it is in contact with the relatively highly electronegative metal lead in an atmosphere which is always charged with sulphuric acid spray seems to make its use for this purpose particularly inadvisable.[1]

[1] Since writing the above, the writer has had an opportunity of carefully inspecting a storage battery room in a large London power-station in which the conductors are almost exclusively made of aluminium rods. These conductors were installed about two years ago and are giving great satisfaction. At first there was some difficulty of the nature indicated in the text of this article but it has been overcome by coating the aluminium with an acid-resisting paint. It is clear that so long as steps are taken to prevent the film of electrolyte which forms over the junction of the two metals from being in electrical contact with both or either of them, galvanic action cannot take place. Vaseline seems to be very effective in this way.

A most interesting and probably very important recent application of aluminium in the electrical industry, based upon its electrical, physical and chemical properties, now claims more than passing attention.

The manufacture of coils, whether for motors, dynamos or other electrical apparatus, involves the insulation of each turn of wire from its neighbour so as to ensure that the current will pass only along the path ordained for it. One of the greatest problems which the designer of electrical machinery has to face is to get a sufficient number of turns into the space at disposal, which is usually very restricted. As has already been shown, an aluminium wire has to be materially larger than a copper wire, so that if it were necessary to insulate it in the way practised in the case of copper wire (wrapping with rubber, silk, etc.), the use of aluminium would be very disadvantageous. Aluminium, however, has a chemical property which has been pressed into the service of the electrician in a most ingenious manner. The surface of the metal is normally covered by a thin, invisible coating of oxide. By immersing the metal in suitable solutions, this oxide film can be increased in thickness until it opposes sufficient resistance to the passage of a current to prevent a short circuit between two adjoining turns of wire in a coil. By this means it has been possible to make coils for electrical machinery which according to Mariage (*Tramway and Railway World*, May 4, 1911) show a saving in weight of about 50 per cent. (which owing to the position of the coils in electrically propelled vehicles is a saving of very great moment) and a reduction in cost of 60 per cent. Moreover, unlike the usual insulating material, being entirely inorganic, the coating made on aluminium is improved rather than damaged by heat, so that the danger of burning the insulation and so short circuiting the coils is diminished. On the other hand, the size of the coil must still be somewhat larger and the difficulty of making effective joints is greater than in the case of copper. Such coils have not been in use very long but their application seems to be increasing very rapidly and the writer is of the opinion that their ultimate adoption on a very large scale is assured.

Space does not allow of a detailed discussion of the use of aluminium in other directions in the electrical industry and mention can therefore only be made of such articles as current

collectors on electric railways, fuses, lamp fittings, meter cases, lightning interrupters, etc., for all of which purposes aluminium is now in use to some extent.

In conclusion, it may be said that the very large development which is taking place in the introduction of aluminium for electrical work represents no mean achievement. Unlike some of the industries which will be considered later, the electrical industry was quite satisfied with copper and did not realise that the advantages which have been enumerated were attainable. It has been led to appreciate them by enlightened propaganda which benefited both the industries concerned.

Transport Vehicles

The rapidly growing use of aluminium in the construction of vehicles is based on several distinctive properties of the metal and its alloys. Before dwelling on these, it will be well to enumerate the actual uses to which the metal is being put. The principal users are the motor-car builders, who have applied the metal to making panels and mouldings of carriage work, in the construction of the jackets and crank cases of the engine and for the gear-box and other casings of the transmission mechanism. Far less general at present is the use of aluminium in railway coach building, in which it is only used for panelling and in still rarer cases for door handles and similar minor fittings. In the case of aerial vehicles, aluminium is used in constructing seats, shields, instrument cases and, in fact, wherever lightness without strength is required. Formerly, aluminium was used in making the joints between members of the frame but this use of aluminium seems to be dying out; the classic cases of the Zeppelin airships and the Barrow airship represent isolated instances of abortive attempts to use aluminium and its alloys for constructional purposes in aerial work.

From the above it will be seen that aluminium is used either as sheet metal or in the form of castings.

Aluminium sheet was originally used for panels on account of its lightness. To-day a more important property of the metal is its extraordinary malleability, by reason of which panels of complicated shapes may be beaten out from it more cheaply than from thin sheet steel, unless a large number of

similar panels are to be made, in which case costly machinery can be installed for the purpose. The surface of a well-made aluminium panel is also better than that of one made of steel, whilst wood, owing to the shrinkage which it undergoes, the amount of paint it absorbs and the difficulty of working it, is no longer used for motor-car work.

The advantages accruing from the use of aluminium for the purposes mentioned are not sufficiently marked, however, to induce makers to employ it unless the price of the metal be very low. It has been seen already that when the price rises appreciably, aluminium is discarded in favour of steel but at prices obtaining at the time of writing, aluminium panels are being used to a large extent.

The case of aluminium castings for engine parts is very different, as the advantages the metal has are very conspicuous and be the price high or low, very little else than aluminium is used. In the first instance, the saving of weight is very considerable, as such castings are of necessity bulky and if made in gun-metal or iron would burden the machinery to an excessive extent. The saving of cost is also very large indeed.

Pure aluminium is not used for this class of casting. When unalloyed, aluminium does not run at all well and in consequence small passages in the mould may not be well filled. Moreover, it often happens that portions of the molten metal which meet in the interior of the mould do not unite owing to the skin of oxide which covers their surfaces. Aluminium itself also lacks the necessary rigidity and the shrinkage of the metal on solidification (1·8 per cent.) makes the production of sound castings difficult. Recourse is usually had to alloys containing about 10 to 12 per cent. of zinc and 2 to 3 per cent. of copper. These alloys have the properties which aluminium itself lacks and are more suitable even than other metals for the production of castings of intricate pattern. If the percentage of zinc be increased to excess, the castings are apt to break when exposed to continual vibration. In earlier days great trouble was experienced on this account but when the enormous number of castings in daily use is borne in mind, the number of breakages now occurring must be considered trifling.

Household and Travelling Utensils

In discussing the application of aluminium to household purposes, travelling and military equipments, properties of the metal have to be considered which are of no account in the cases previously considered. The use of the metal for such purposes depends in the first instance upon the fact that compared with the materials heretofore used in kitchen and camp, aluminium is either infinitely safer from a hygienic point of view or far more durable. In this case, comparison lies between aluminium on the one hand and iron, copper, enamelled iron and tinned iron on the other. For heavy cooking utensils, such as large kettles and heavy pans, iron still holds the field. Iron vessels, however, can be used only for a very limited number of purposes and are unsuitable for general use owing to the difficulty of keeping them clean and free from rust and the coloration and taste they impart to food. Their thermal properties also render them unsuitable for fine cookery.

Copper cannot be used safely owing to the solubility of copper in the weakly acid materials contained in foodstuffs and the fact that copper salts are most active poisons. Copper vessels, therefore, are coated with a thin layer of tin. This precaution is by no means sufficient to eliminate the danger, because the tin, sooner or later, wears off. Moreover, the cost of copper vessels is more than the purse of most housewives can bear and the cost of retinning is a permanently recurring charge. In point of price, aluminium cannot bear comparison either with tinned or enamelled iron but the life of the former is so very short that it does not form a serious competitor Enamelled iron may and frequently does give satisfaction on this score; on the other hand, it is entirely untrustworthy and in case of damage to the enamel it is the most dangerous material which can be used. There is in this case no question of poisoning as with copper but chips of enamel become intermixed with the food and probably are the cause of disorders such as appendicitis, etc., more frequently than is supposed. From all these disadvantages aluminium is absolutely free. Drawbacks of its own it has but these are distinct from those cited above. Aluminium is second only to copper among the common metals in thermal conductivity and gives no colour to the finest materials. Dirt is seen so easily upon its white

surface that it is possible to tell at a glance whether it be clean or not. In addition to the fact that it dissolves but slowly in weak organic acids is the immensely important fact that even in solution it is entirely innocuous. Unlike tinned copper, tinned iron and enamelled iron, it is uniform throughout its thickness and consequently there is no coating to wear off, crack or chip. Having these advantages, the question may well be asked "How is it that its use is not universal?" The reasons are three in number. The cost of aluminium still places it above the reach of the poorest; the aluminium formerly used for the purpose was inferior; lastly, the metal cannot be cleaned by the methods generally in use in the kitchen.

The first of these statements need not detain us.

The inferior metal formerly and still occasionally used brought and brings its own nemesis. It was stated above that the solubility of aluminium in weak organic acids is small and if it took place generally over the surface of the metal, it would be negligible. Unfortunately, however, this is not the case. The presence of small impurities in the metal or even of physical differences between adjacent particles, may lead to local dissolution and pitting or perforation of the metal. Owing to improvements in the methods of manufacture, the former trouble has been largely obviated and since the recognition of the importance of the physical state of the metal, still further improvement may be looked for. As a matter of fact, the degree of progress which has already been attained is very remarkable. In America and in Germany, millions of cooking utensils are made annually and the percentage of returns is nowadays very small indeed.

Now as to the third difficulty.

Aluminium unfortunately is readily attacked by alkalis and, therefore, the cleansing agent of the kitchen, soda, is one of its worst enemies. As a consequence, the cleansing of aluminium must be effected mechanically and entails appreciably more labour than if effected in the customary manner by means of alkali. This is the chief difficulty which remains to be overcome. In Germany, where the "hausfrau" herself takes pride in the appearance of her kitchen and herself does much of the cooking, it has not been sufficient to counteract the obvious advantages the metal has. One German factory known to the writer used in 1910 about 3,000 tons of aluminium, almost all

of which was made into kitchen utensils. In the United States a huge business has been built up, mainly by the exertions of university students, who in their long vacation were engaged to educate the public to appreciate the advantages of aluminium; whilst even the Indian "ryot," who has always cleaned his pots and pans by polishing them with sand, is rapidly learning to substitute aluminium for the brass bowl prescribed by immemorial custom. Only in England is progress slow, mainly, in the writer's opinion, because in England the housewife does not cook and is not mistress in her own kitchen—where she walks in fear and trembling—and because no one has arisen who has had the courage to undertake the education of our national institution, Mary Ann.

In addition to the advantages cited above, the lightness of aluminium is the cause of its wide use for the field equipment of soldiers and travellers, to whom every ounce saved in the weight of water-bottle and cooking-pot is of importance. Moreover, the malleability of the metal renders it practically unbreakable, a factor of no small consequence when the treatment to which field equipment is subjected is borne in mind.

Chemical Plant

The properties which have rendered the success of aluminium in the kitchen possible are also those upon which its claims as a material for the construction of chemical plant are based. This is true more especially of apparatus suitable for use in foodstuff factories, which have been erected in such large numbers during the past two decades. A modern jam factory, an extract of meat factory, a cordial factory, is each but a domestic kitchen magnified a thousandfold, a well-equipped condensed milk or margarine works being but the apotheosis of a dairy, where purity of taste and colour and freedom from infection must and do reign supreme.

The chief attraction which aluminium offers to the maker of foodstuffs lies in the fact that aluminium salts can be consumed in considerable quantities without exerting any injurious effects whatsoever upon the human system. If this fact be of importance in the kitchen, it is immeasurably more important in the factory, as the introduction of poisonous material derived from the apparatus used on a large scale would endanger the

health of thousands of people, to say nothing of the reputation and consequently the success of the manufacturer. Many metallic salts, especially those of the heavy metals, act as specific poisons on the human system but aluminium salts have no such deleterious property. As it is not absorbed by the mucous membrane of the stomach, aluminium has no chance of exerting any objectionable effect, being rejected by the system *in toto* (*Siem. Dissertation Dorpath*, 1886; *Plagge u. Lebbin. Veröff. des Militär. Sanitäts Wesens*, 1893; Herbert, Acad. des Sciences, 1907, etc.). Thus it comes about that even if aluminium were readily dissolved by foodstuffs, it would nevertheless be harmless. But we have seen already that aluminium is but very slightly attacked by weak organic acids, so that the use of this metal is attended with complete safety.

In this respect, aluminium stands pre-eminent among the metals, its only rival being perhaps tin, which owes its popularity to its insolubility in weak acids. Once formed, however, its salts are poisonous. But tin, in addition to its costliness, is deficient in the mechanical properties which are required for the construction of large chemical plant. It is, therefore, generally used only as a thin coating on other metals—copper, lead or iron.

In this connexion, reference may be made to one particular living organism towards which aluminium is entirely inactive, namely, yeast, a property which renders the metal one of extreme importance to brewers. Brewing is an industry which is carried on under the most stringent conditions of asepticism. The demands made upon the modern brewer are such that foreign organisms of every kind have to be excluded rigidly, the most painstaking precautions being taken to attain this end. One of these is the gradual elimination of wooden vessels, which on account of their porosity tend to harbour bacteria and are cleansed with difficulty. In place of wood, both copper and enamelled iron have been used but it has been found (Chapman, *Journal of the Institute of Brewing*, 1909) that under certain conditions copper is dissolved by beer worts at the air-water line and that the copper which enters into solution affects in a very marked extent the reproductive power and fermentative vigour of yeast. Enamel is free from this objection but it is easily damaged, so that the iron subsurface becomes exposed and rusts, so affecting both the taste and the brilliancy of beer.

Chapman has shown that the salts of aluminium do not affect

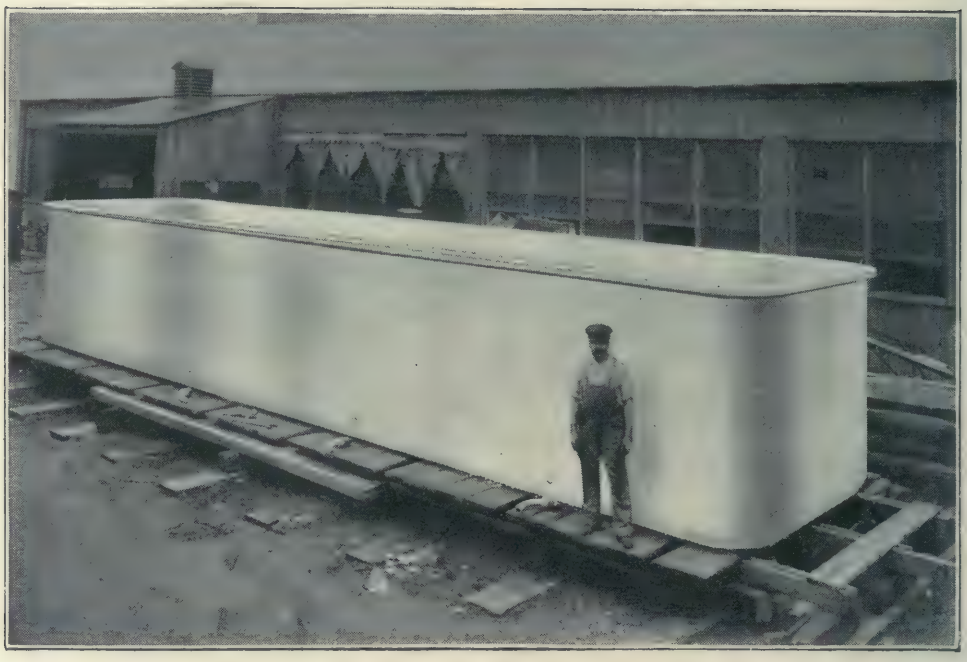


FIG. 1.—A fermenting vessel made of aluminium sheet for an American brewery.

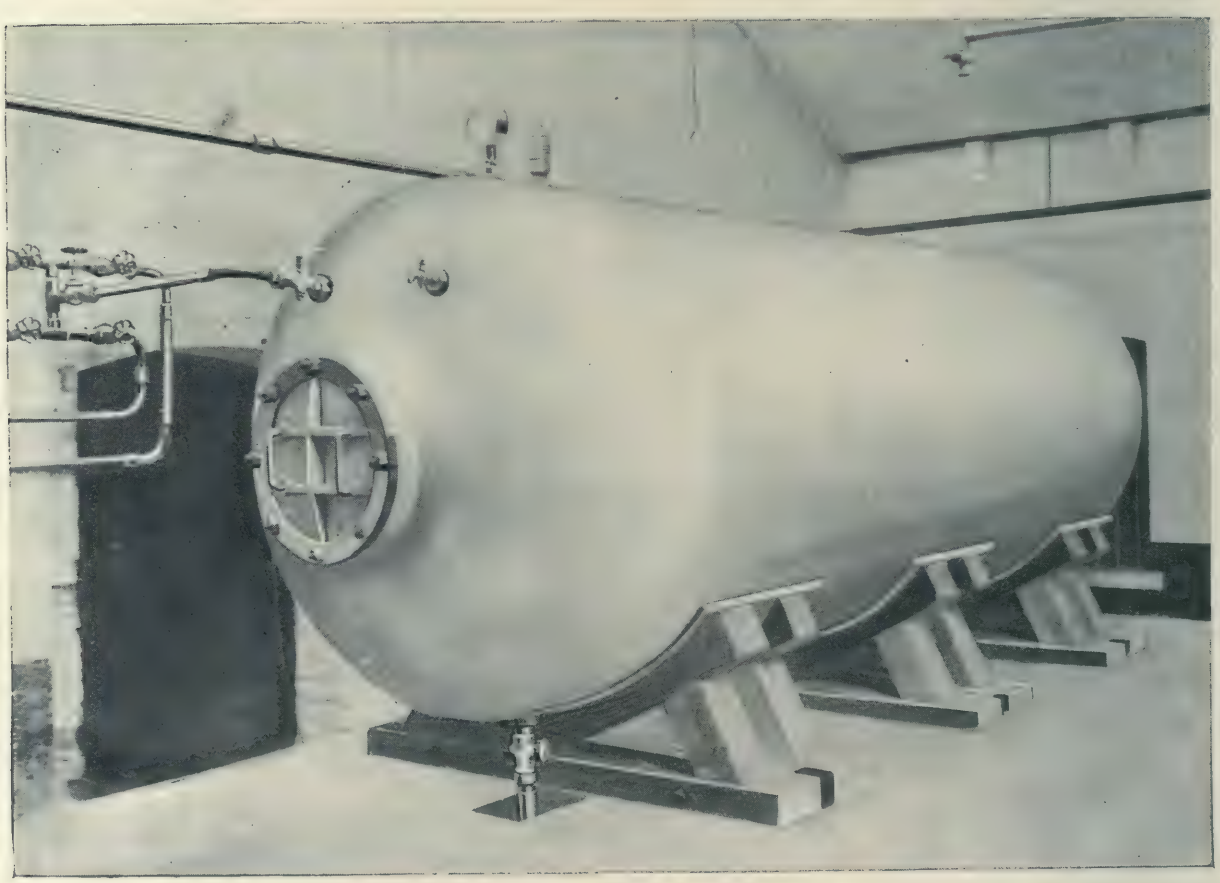


FIG. 2.—An aluminium pressure vessel having a capacity of 1,800 gallons and made to withstand a pressure of 45 lb. per square inch.

yeast in the very least (*ibid.* 1911), the yeast cell disregarding the presence of aluminium salts exactly as the human stomach does. This neutral property of aluminium, taken in conjunction with the fact that its solubility in sound beer is extremely small, have led to the rapid introduction of brewery plant made of aluminium.

The vessels used for this purpose are of exceptional size, some of them holding 25,000—30,000 gallons of beer. To manufacture such tanks of aluminium was quite beyond the capacity of manufacturers until very recently but new processes have been evolved (to which reference will be made later) by which vessels of this size can now be made and a new industry based thereon has arisen. The accompanying illustrations show both a fermenting tank made of aluminium and also a vessel in which secondary fermentation takes place under pressure, the carbon dioxide evolved being imprisoned and dissolved in the beer. These tanks, which are being used in rapidly increasing numbers, have to withstand considerable pressures, so that their manufacture places no small demand upon the skill of the makers.

The second consideration which has led to the adoption of aluminium in many cases is the fact that the salts of aluminium, without known exception, are colourless. In this respect it differs from practically all the other common metals used by chemists. All the salts of copper and iron are coloured, whilst both lead and tin easily give rise to discoloration. In many industries this fact is of great importance, as it is often impossible to prevent dissolution altogether and in such cases the certainty that no discoloration will follow from the introduction into a product of small quantities of the metal of which the containing vessels are made is an obvious advantage. Many examples might be quoted in illustration but two or three will suffice. In making varnish, various gums, oils and resins are heated in small metal pots, over a direct fire, at temperatures varying from 350 to 400° C. At this temperature, the lower boiling constituents distil off but condense in part on the upper and cooler surface of the vessel, thence flowing back into the main bulk of material; these volatile constituents often attack metals very freely and if, as has been the practice, iron or copper vessels are used, the condensate is brightly coloured—brown or green, as the case may be. As one great object of the varnish maker is to obtain a

colourless product, the use of aluminium is remarkably advantageous. The comparative weakness of aluminium at the high temperatures which are customary in varnish-making was at first considered to be a drawback to the use of the metal but in practice it has been found that this is more than offset by the fact that aluminium is unaffected by furnace gases. The sulphur compounds of aluminium are only formed with great difficulty and as it is the sulphur from the coke fires which plays havoc with copper and iron varnish pots, this cause of deterioration is inoperative in the case of aluminium.

The second case to be examined under this heading embraces industries such as those engaged in the manufacture of jams, preserves, fruit and other essences, in which materials are used which either contain acids or from which acids are easily produced. Jam boilers, for instance, use copper boilers. Although the amount of copper dissolved by fresh fruit is insignificant, if left uncleaned a jam pan will acquire a green coating of copper salts in the course of an hour, owing to the attack of the copper by the acids in conjunction with the oxygen of the air. This difficulty obtains in a number of trades and in some cases, as, for instance, the distillation of the higher fatty acids, it is necessary to reject, owing to their bright green colour, the first runnings from the condensers if the still has stood idle for a few hours. Here again aluminium offers a definite advantage, combined of course, in the case of materials destined for human consumption, with the fact already referred to, that no poison can gain an entry.

There now remains to be considered the fact that a number of chemicals which attack most metals vigorously dissolve aluminium with extreme reluctance. The most striking instance of this kind is indubitably that of nitric acid. This material, generally considered to be the most corrosive of chemicals, attacks aluminium so slowly that pipes, tanks, hoods and even pumps of the metal are used on a large scale both in the manufacture of the acid and in the manufacture of explosives in which nitric acid is used. In the process which is now being worked on a very large scale, whereby oxides of nitrogen are produced from the air by electric discharges, the cooled gases are conveyed through aluminium pipes to the towers in which nitric acid is produced.

Sulphurous acid and sulphuretted hydrogen are similarly

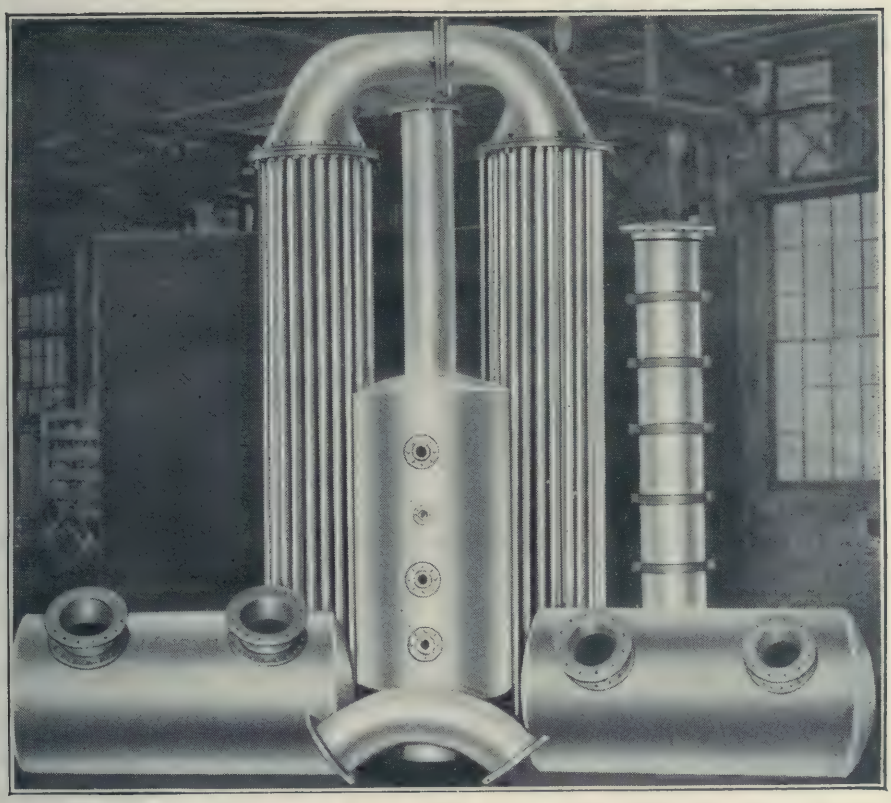


FIG. 3.—Parts of two aluminium vacuum stills, condensers and receivers for the distillation of fatty acids.

without effect on aluminium but advantage has not been taken of this fact to any great extent at present.

Of far greater importance is the fact that of all the common metals aluminium is the least attacked by the higher fatty acids of which the bulk of the vegetable and animal oils are composed (G. A. Emery, *Report of the U.S. Dept. of Agric.*, p. 265, 1909). Immense quantities of these oils are now converted into foodstuffs in the form of margarine and similar products and it has been found that aluminium is the most suitable metal to use in the construction of the plant required. Not only do the advantages of colour and immunity from toxic effect apply in full force but the metal is actually unattacked by these materials, some of which, such as oleic acid for instance, corrode other metals with great rapidity.

In the foregoing account no attempt has been made to enumerate all the uses to which aluminium is being put in the chemical trade; only a few examples have been selected to illustrate properties of the metal of which advantage is being taken in various industries. Though these properties have long been recognised, aluminium has been used by chemists only to a very limited extent on account of the difficulty of uniting sheets of the metal, which until quite recently prevented the manufacture of any but small articles of aluminium such as domestic utensils or such chemical vessels as could be made in the shape of castings. Aluminium is the most electro-positive of all the metals used in the industries and in consequence suffers corrosion when in contact with other metals with extraordinary readiness. Inventors of solders for joining aluminium have invariably split on this rock. It is usually considered impossible to solder aluminium but this is by no means the case. Aluminium can be soldered quite readily but the soldered joints corrode rapidly owing to the fact that electrolysis is induced at the joint by the contact of the metal and solder. On the other hand, aluminium cannot be riveted so well as other metals, on account of the softness of the metal, a difficulty not readily overcome; the rivets are apt to draw out, giving rise to leakage, especially under the influence of temperature variations and vibration. Quite recently means have been found to apply the lead burner's method of jointing to aluminium. A lead burner fuses two sheets of lead together without the interposition of any foreign metal. The

same method has now been adopted for the jointing of aluminium, the only difference being that fluxes are required in the case of aluminium to remove the oxide film with which the surface of the metal is normally coated. Although apparently so small a point, it has been thought well to lay stress on this matter because of the consequences with which it is fraught. Until the method of jointing by autogenous soldering was perfected the use of aluminium in the chemical industries could not be made general. By its introduction a weapon has been forged which has enabled those who have devoted themselves to the solution of the problem to place at the service of the chemist and chemical engineer a metal with which, on account of its special properties, many grave difficulties affecting the use of other metals may be overcome; above all to render the manufacture of all materials destined for human consumption safe and hygienic. The difficulties which have so long stood in the way of the manufacture of aluminium apparatus on any but the most restricted scale, in fact, may now be said to have disappeared and the photographs which illustrate this portion of the article are sufficient evidence that any apparatus which the chemist may require to have made in aluminium is now readily obtainable.

The author wishes to express his thanks to the British Aluminium Co. and the Northern Aluminium Co. for permission to use the photographs shown herein.

OXFORD AND GREEK

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Oxford's rejection of yet another proposal to dispense with knowledge of Greek in the case of certain classes of candidates for the B.A. degree offers occasion for brief retrospect and some reflections on the whole "Greek Question." As one of the newspapers put it, after the voting last November, "It will be a year or two before Greek is saved from its Oxford friends." As a matter of practical politics, the question is shelved for the moment; shelved indeed in all probability till Oxford itself can be "saved" from constitutional obstacles and diverted from the plausible pastime of "reform from within." But the question is not postponed for long; nothing that official Oxford is likely to do can alter the course of events elsewhere.

The discussion has turned of late mainly round two large problems which taken together *are* the "Greek Question" in its present phase. One of these questions is theoretic—"Is a good general education possible without Greek?" to which belongs the supplementary question, "Is it still a good general education, if the only Greek which it includes is Greek for Responsions?" The other is practical: What position does Oxford wish to occupy—and therewith, what position ought it to occupy—among other universities of the Empire in respect to those whose education, though good enough and general enough for all other universities except Oxford and Cambridge, yet has not been either so "specialised" as to guarantee real knowledge of Greek in boys of eighteen or so deranged as to admit the modicum of crammed accidence which Oxford and Cambridge accept as equivalent? The two questions are obviously quite distinct. Oxford has long since overcome the momentary irritation with which it first heard itself described as the "home of lost causes": many prominent Oxford men, both resident and non-resident, have come to

take a perverse pride in the varied contents of their asylum. Experience gained anywhere else is habitually held to be inapplicable; "I am sure it would never do here," is the first line of defence against reform: and the reformer admits with shame that usually it wouldn't.

It is a notable sign of the course of events elsewhere that, within a month of the vote in Oxford, the Board of Education was ready with an exhaustive report on the working of the examination system in Secondary Schools. It is another sign, that in the last few days a Royal Commission is announced to inquire into the methods of appointment to (and promotion in) the Civil Service, the upper divisions of which offer one of the few remaining careers for which the classical courses at the older universities are popularly regarded as adequate and "safe" preparation. So intimate, indeed, is the correlation become between these university courses and the highest present requirements of the Civil Service Commissioners, that hardly any change in these requirements could be contemplated which would not derange both the teaching and its academic tests and even the financial arrangements of colleges. It is not only "Smalls Greek" or the curriculum of the Secondary Schools that is on its trial; the old universities are called to account for their husbandry of the nation's brains.

It is not very easy to say why the Greek question should give rise to divergences of opinion worthy of a dispute not in pedagogy but in politics or religion. But the sharpness of the antithesis is partly explained when we remember that it is one of those between *Have* and *Have-not* on which, as Plato observed long ago, no agreement is possible, because one of the parties is *ex hypothesi* incapable of realising the position of the other. It is, moreover, almost as hard to persuade the man who has Greek and loves it, that any reason is left in the man who hates it having none, as to persuade the Greekless fanatic that the Grecian has not been made mad by his much learning. And it is small consolation to the practical man to be reminded by the historian that the feelings now aroused on both sides are as nothing, compared with the passions and open violence which ensued when Greek first came into vogue in Oxford as a new and optional alternative. In those days, as now, the arrival of students with a different kind of preparation meant that some teachers had to remodel

their teaching. Then, they had to square their Latin quotations of Aristotle with the newly printed Greek original. Now, they are afraid of having to lecture on Aristotle in such a way as to be intelligible to the Ruskin men who read him, as they read Moses and Matthew and Marx, in the vernacular. Painful and sad, that these things should be; but what a satire on a place of "learning and education."

Nor is the gulf between the Grecian and the modern "Trojan" really filled by the discovery, which I think is a real one, of another pair of contradictories—a small but growing class of persons who though they have Greek and love it, do not love it so well as to insist upon casting it like pearls before swine; and another, at present smaller class—but one which I think grows even more rapidly—who, though they have no Greek themselves and in great measure because they have none, realise that those who have it have something better than pearls and desire earnestly that others whom they know to be better than swine should have opportunities—hitherto withheld—of acquiring it.

The Plaintiff's Evidence

I have never met any one nor anybody who had heard of any one who was really proficient in Greek or chemistry or shorthand either and yet disliked the subject of his proficiency. So I venture to assume forthwith that it is not "Greek" itself to which people object but something in the process by which proficiency in "Greek" is acquired: either the process itself or (what really amounts to the same thing) some disproportion, in their own experience, between the effort and the result. People do not attack Greek-in-general as bad. What they regret is the ill success of their own attempts at Greek; and the reason why they regret them is simply that these attempts have failed.

But in education there are two parties to failure, the taught and the teacher; and since, again, I have never met anybody who knew of a man who had taught *himself* Greek and regretted it, I take leave to assume as probable that when a man has been taught Greek by some one else and has survived to regret it, the blame is not to be put upon the learner only. I would even go further and suggest that it is probably because there is something the matter with our teaching of "Greek" that so

many people, who have failed to learn it, abuse "Greek" and the Grecians, instead of regretting their failure and trying again.

It is a common observation among compulsory Grecians that few if any who can pass Responsions in Latin are really incapable of passing in Greek; and we have only to compare the papers currently set in each subject to see that this is inevitable. With the sole exception that in Responsions composition in Greek is hardly required, the two languages are expected to be handled in precisely the same way. Yet in spirit, in handling, in their whole method of expressing thought, in all, indeed, but the barest accidence and vocabulary, the two languages are as different as French is from German or either of these from English. When, therefore, I find that many objectors to Greek are content to leave a very large place open for Latin in their reformed curriculum, I begin to suspect that what they dislike is not really Greek but a misguided attempt to teach Greek as if it were a subsequent, supplementary and superfluous thing; a mere duplicate of Latin and a poor one. I am somewhat confirmed in this view by a criticism which sometimes reaches me to the same effect on the teaching of French and German; and I am reminded of a young Greek with whom I once travelled, who was trying to learn French and English simultaneously by the same "comparative method." Whenever he addressed me in either language, I was unluckily certain to mistake his intention and to reply in the other. If Cicero were ever condemned, for Elysian misdemeanour, to examine in Responsions or in some Sixth Forms, I fear he would have much the same difficulty, at all events in syntax and idiom.

Now this muddle of method is no fault of the pupil. No one who started to learn Greek unaided and went as far as to spell out simple Greek narratives would dream of writing Greek sentences with the words in Latin order, which I find to be a common habit. On the other hand, this is exactly what we should expect to find, if two other things were true: (1) that the teacher did not much mind *how* he taught Greek; (2) that he did not really know much about the Greek language beyond the accidence and vocabulary. Above all, we should suspect that he had an untrained ear and that the reason why his ear was untrained was because he did not himself *speak* either Greek or Latin fluently and consequently had very little

experience of *Greek speech*; that his notion, in fact, of "teaching Greek" was to cram the boys' memories with a particular kind of algebraic notation in which ϕ, for example, was a function of π and the terminations of nouns and verbs were co-ordinates. I do not think I am stretching language unduly if I interpret this first group of objections to "Greek" as really amounting to this: that a great deal of what is taught as Greek is not the live language at all but partly a poor imitation of Latin and partly *gibberish*.

A second popular objection, heard rather from parents than from pupils, concerns not the form but the matter of the "Greek" that is taught. Conceive a German school in which the boys' knowledge of "English," as a character-forming "guide to life," was based exclusively on *King Lear*, *Measure for Measure*, Berkeley's *Alciphron or the Minute Philosopher* and Burke's speech *On the Nabob of Arcot's Debts*; in which Spenser, the Authorised Version and particularly Burns were under a taboo (like Herodotus and Theocritus) on the ground of "dialect" and "style"; and in which all English history and social life outside the "subject matter" of the "set books" were excluded as far as possible, especially from the higher forms, to make time for the writing of blank verse and common-metre hymns. Then, for *King Lear* and *Measure for Measure* write Greek problem-plays like the *Medea* and *Alcestis*; for Burke, write the *Olynthiacs* of Demosthenes; and for *Alciphron* write *Euthyphro* and *Crito*, little essays, graceful enough, on the limits of piety and loyalty but repulsive as a tract by Hannah More to the normal British boy. "Why," asks the bewildered parent, especially if he can spell out some of the "set books" for himself, "why do universities prescribe works like these to test what they must surely know by this time to be the merest beginners in Greek?" No wonder, either, that the average boy comes to the conclusion that, in the words of Mr. A. C. Benson,[1] "classics of all kinds were written by people who meant nothing in particular, for people who wish to read about nothing in particular and to feel superior while they do so"; or that "if like many boys he is quite ignorant of English literature, he comes to the conclusion" after comparing his Greek play with his pidgin-English crib "that literature in general is devoid alike of sense and interest."

[1] *Times*, November 29, 1910.

Administered even in very small parcels, Greek literature, like all great literatures, like Greek art and like all human masterpieces, makes indeed its appeal to our common humanity. But it gives its message in its own way. It has its own forms, its own conventions and pre-suppositions, intimately correlated with the social circumstances of its growth. To neglect these and confront boys, brought up in what (for all our Hellenistic varnish) is really quite a different kind of society, with the bare elements of a language and a few short isolated bits (however splendid) of a literature and no background at all, is like trying to teach the art and the critical enjoyment of painting by lessons in brushwork and three or four "set pictures" selected each from a different Italian school; or to teach chemistry by elementary glass-blowing and three or four "set elements"—say carbon, iron, gold and phosphorus—selected likewise so as to "cover the ground" and be as incoordinate with each other as possible. The result could be foreseen, if experience counted with dons. So miserable is the classical equipment of the average undergraduate, in almost all departments of Greek literature, thought and life—quite apart from the question how far he takes interest spontaneously even in such "Greek" as he knows—that I confess to a perennial surprise that he acquires as much as he does even of mere "grammar and language." The scrappy subject-matter of his "books" seems to me so much mental rubbish.

Now the reason both why he seems to the examiner to know so much and why, outside narrow limits, he has yet really learned so little, is that since "cram" is quicker for the pupil and easier for the teacher than "education" of any sort, the attempt of the older universities to "save Greek," by making it compulsory in Responsions, enforced the substitution of cram for teaching, as soon as the requirements of other public bodies began to limit the time available for Greek at all. Exactly the same disaster has befallen the preparatory schools for upper-class boys, because until quite recently the great public schools still exacted the same standard of Greek as it was natural to require when hardly anything but Greek and Latin was being taught to little boys at all. Admirable elementary grammars are still printed and sold to little boys; but at one school of my acquaintance the boys are actually instructed to strike out the explanatory matter in them, lest their attention should be dis-

tracted from learning the examples by heart. Teaching Greek and Latin thus, no master needs to *think*, any more than do the boys. It was lately given me as the reason why an apprentice-master in a small school could be expected to combine his own reading for a university degree with thirty-six hours a week of class-work, that "of course care is taken to give him classes which need no preparation out of school." Yet this master is supposed to be learning how to teach. Shall it surprise us if he fail to teach boys how to learn?

When the methods and conditions of Greek study can be like this, what wonder is it that the parents of average boys and even the parents of boys above the average level, increasingly protest not only that their boys learn nothing in the "Greek" hour but that in many Greek-teaching schools the intellectual tone is lower, the concentration of interest and energy upon games and trivialities more marked, their own difficulty more grave, at the last, in finding anything that the boy can still do, still less anything that he *wants* to do, to earn a living. To paraphrase only slightly a sentence already quoted, such "classical" education is conducted "by people who mean nothing in particular for people who wish to do nothing in particular and to feel superior while they do so"; whilst the classical-trained boy, "if like many boys he is quite ignorant of English" life, in its historic and geographic setting, comes to the conclusion that life in general "is devoid alike of sense and of interest." As taught to him, the languages and literatures with which his studies deal are indeed "dead languages" and "dead letters"; but they are dead for him, only because they have been killed in the attempt to teach them. Yet this is what the people who "save Greek" persist in describing as an ideal "preparation for life."

But we have to face the facts. From a large majority of what an earlier generation called its "Grammar" Schools, Greek of the traditional sort has been abolished, grammar and all; and abolished in my opinion rightly. Real Greek was no longer being taught at all; and the substitute which was being crammed into the boys was not wanted. The masters who thought they were teaching Greek were sowing the seeds of a heartfelt hatred of the whole subject, of which we reap the harvest now an hundred-fold. And Convocation loves to have it so!

We may easily agree with both sides when they demand a

"preparation for life" which shall really be "of use in the future career." But for the very large majority, what is "of use in the future career" is not Greek, any more than it is chemistry; except in so far as Smalls Greek is "of use" as a key to open Oxford with. There are probably about the same number of real chemists in this country now as there are real Grecians; of both the supply is regulated partly by the demand but mainly by the success of real teachers in arousing real interest in a real subject. But that "interest in reality" which is the one thing "useful in the future career" is as rarely elicited by Greek as by chemistry, when the conditions prescribe cram as a makeshift for teaching. As Sir E. Ray Lankester has urged very forcibly,[1] it was because "the study of Greek was the study of science" and the surest guide then known to an interest in realities, that it was properly made compulsory in the Revival of Learning against enormous opposition from anti-scientific obscurantists. Now "it is because the study of Greek is no longer the study of science that Greek must cease to be a compulsory subject." "Science must be taught independently and of set purpose, without Greek and as a primary and compulsory subject"; because "for the cultivation of the mind," which is regarded by him no less than by his opponents as an end in itself, "an initiation into the working of natural science is as indispensable as an introduction to Greek literature." In which commonsense view Sir E. Ray Lankester has been sadly anticipated by Plato.

It is worth noting, perhaps, at this point that modern modes of studying Greek literature and Greek life are themselves the result of applying to Greek subjects methods of inquiry which were formulated and applied in the natural sciences two generations ago and are now being transferred, late and partially, to human studies. And it is our neglect to train teachers destined for classical schools in the methods of natural science which has prolonged a pre-scientific age so disastrously in just those places where it ought to have been earliest and most ruthlessly curtailed. In the same way, the neglect to train teachers for these schools, either in scientific methods of language-study or in the modern art of language-teaching or indeed to require from them any formal training for their profession at all, is a main

[1] In his address to the Association of Public School Science Masters, *Nineteenth Century*, March 1911.

cause why in most public schools and among the majority of boys in them all the level of intellectual interest is so low. It is indeed, I think, far lower in them on the whole than in most elementary schools, where professionally trained teachers are now in a majority. In the great public schools, as the Head Master of Eton knows,[1] "the main cause of muddle-headedness and hatred of learning lies in the unsoundness of the foundations of language-study and mathematics." And he has no doubt about the cause. "This unsoundness is due to the haste with which the foundations are laid, inevitable when an excessive amount is required at the age of eighteen or nineteen."

The Greek Question then turns out to be in the main an extreme case of the much larger problem of providing teachers and teaching which are alive and enlivening. "Greek" is worse taught and consequently worse learned than most other subjects; and one reason for this is that the Greek teachers are themselves the prime product of pre-scientific teaching and learning and less contaminated by modern scientific thought than any other body of teachers.

The key to this problem is in the hands of the older universities. It may have to be wrested from them but in the meanwhile it is there. The freedom is theirs and the responsibility to set things right. They can shut their doors on Greekless persons, if they please and if (which Heaven forbid) Whitehall do not step in to prevent them; but if they do no more than that, above all if they leave their entrance tests as they are, they are shutting the door not only on the Greekless but on thousands of would-be Grecians whose parents at present send them to great schools to learn Greek, only to find, too late, that instead of learning Greek, they have been crammed for Responsions.

Two distinct issues, therefore, depend on their decision. One is the fate of the Greekless, for whom, if they are not to be admitted to Oxford or Cambridge, other provision must be made; which means that the actual rift in English education between an upper-class and a lower-class system is to be extended through the university stage. This is the compulsionist's remedy. In the elegant language of Mr. Stuart Jones, an Examiner in the Final Classical School at Oxford, "there are universities enough and to spare in which if it be the will of

[1] *Oxford Magazine*, May 11, 1911.

those who would 'dehumanise' education, as they are dehumanising political and social life, a new type of education may be evolved by experiment."[1] The President of Corpus agrees, though less explicitly, "to every one who is content with natural science and modern literature, doors are open in every part of the United Kingdom. Should not the universities of Oxford and Cambridge still be reserved for those who believe that the most scientific mind is one that is not purely scientific."[2] And since November last the ostrich-cry in Oxford has been that of "differentiation of function between the universities of the country," coupled with dark hints of a Royal Commission, to be promoted in Oxford, to "dehumanise" Bristol and Birmingham and "differentiate" Liverpool from Manchester.

The other issue is the fate of the Greek-teaching schools: for their whole structure and curriculum must be recast one way or the other, according as the old universities enforce bifurcation or no. The present state of uncertainty is bad for everybody but it is worst of all for them. I quote the Head Master of Rugby.[3] The question whether a particular boy who has already begun Greek shall go on with it as part of his school training "must be answered at latest before he is fifteen and there is only one way of answering it honestly, namely, with an eye solely to the boy's educational prospects." "It is the answer to this question and not the first university examination which in reality decides the fate of Greek." But compulsion puts the cart before the horse. "At present, we must consider first, not whether a boy is to profit by Greek but whether he is going to the university," that is, to one of the two universities which require "Greek." "Hitherto, by an examination which in its present form hardly serves the best interests of either the language or the learner, the university has created a large number of sham students of Greek. If the Statute passes, some modern-side candidates will be relieved of a thoroughly bad piece of work and the language will suffer not at all." On the last point, the Head Master is supported by the Professor of Greek at Oxford, who has most clearly expressed his conviction[4] that "classical teaching has suffered in the past and was still

[1] *Times*, November 22, 1910.
[2] Pamphlet, *The Preservation of Greek at Oxford*, November 1911.
[3] *Oxford Magazine*, May 11, 1911.
[4] In Congregation, May 1911.

suffering enormously from the dead weight of unwilling men who were dragged into it"; and also by the Professor of Classical Archæology, who is clear[1] that "Greek influence must be maintained rather by appreciation of its intrinsic nobility than by compulsion." The man in the street takes the same hopeful view. "It would be unwise to display a rigid adherence to the principle of compulsion in education, which is the worst enemy of successful teaching."[2] "There would be more Hellenists and better Hellenism if Greek were voluntary."[3] I hope to show, later on, that it is just where there is no compulsion that there is, at the moment, the clearest indication of a revival of Greek studies.

The Case for the Defence

In defence of "Greek," it is commonly urged that the Greek-teaching schools turn out the best boys; and that this is due to the fact that they teach Greek. The first point cannot be tested unless and until the boys from Greekless schools have equal access to university courses and through them to the professions and other careers which are assumed (again without proof) to be frequented only or mainly by "the best boys." Greekless persons not unnaturally object that their inferiority is unproven as regards either intellectual ability or that other asset of "character" in the formation of which the Grecians claim that "Greek" plays so essential a part. They appeal to the large preponderance of Greekless men among the responsible leaders of trade and the industries and to the proverbial lack of economy and alertness in great Government departments, which have hitherto been rather close preserves for "Grecians." They contend further—and their contention has been ably re-stated by a recent correspondent of the *Times*[4]—that it is not the "Greek" but the wealth of the Greek-teaching schools which attracts to them the "best boys"; that the "best boys" might be better still, if the "fashion" at these schools served them at all as well in matters of intellect as in morals and tailoring. They do not see why schools conducted (in a memorable phrase) "by gentlemen for gentlemen" should ascribe their characteristics to

[1] *Morning Post*, June 28, 1911.
[2] *Times*, November 30, 1911.
[3] *Globe*, November 29, 1911.
[4] *Times*, January 2, 1911; reprinted in *Classics and the Average Boy*.

"Greek," when it is only too clear to bystanders that the training which they give is not Greek education at all (as Plato for instance understood it) but the good old Persian prescription and very little else—"to ride and to shoot and to tell the truth." The Greeks, as Sir E. Ray Lankester has reminded us, prescribed no "compulsory cuneiform," however much they believed their culture to rest on the Wisdom of the East. They did insist, on the other hand, on intimate acquaintance with their own literature; music for them, as an educational subject, was a blend of harmony and acoustics; and their humanists tempered athletics with all the sciences that there were.

Secondly, some people still remain so far outside the current of educational thought as to defend the study of Greek on the ground that it is "discipline," tending to the formation of "character." No one denies that character is formed by effort; and that some subjects demand more effort than others from average minds. "Tell Harry that Latin *is* hard," wrote Mr. Gladstone about his son at school, "but it is just because it is hard that it is so well worth learning." But there is effort and effort and it is little use to expect boys to learn to make the right sort of effort as long as the efforts which they are led to regard as most fundamental in their schooling are directed to ends beyond their understanding and carried on by processes which they know to be foolish and futile in any pursuit out of school. Did anybody ever learn to play football or cricket by memorising lists of irregular kicks or rules for composing an eleven?

But the strongest stock-argument for "compulsory Greek" is that unless the universities maintain the present form of compulsion "Greek will die out"; the suggestion being that if Greek die out in England unmentionable evils will follow. These evils being unmentioned are not easily to be discussed in this article. But will Greek die out? Surely Oxford and Cambridge, serious institutions as they are, are taking themselves just a trifle too seriously. Are they not using precisely the argument which will convince the enemies of "Greek" that if they can but force the "last ditch," as it is called locally, the "Greek" which they hate will be extinguished for good and all; and also that if the last ditch did not defend this particular stronghold from invasion no one would care much about anything Greek.

The theological argument which is used now and then in Convocation is only a special case of the argument that "Greek

will go." What Oxford itself thinks about theological Greek for laymen, let "Divvers" testify, the blasphemous farce which—thinly disguised in the Statute Book as a "First Public Examination in Holy Scripture"—still disfigures the career of Science men and Greats men alike; with its statistics about miracles and its pretence of testing knowledge of Hellenistic Greek by translation from books of which the "Authorised Version" is in every bedroom. As for the Church, "unless the universities declare Greek to be imperatively necessary," says the President of Corpus, "a body of clergy will grow up unable to instruct their people in the interpretation of the New Testament from a first-hand knowledge of the Greek text." Well, the Church owes a good deal to Oxford and might fairly be asked to show a little backbone, shoulder its own risks and give Oxford courage in a very difficult crisis. Whether Oxford owes much to the Church, particularly in the last two centuries, is open to more dispute; it seems to spectators that if the proportion of clerical to lay voters in Convocation had not been (for irrelevant reasons) so high, the call to "save Theology" by helping to "save Greek" might not have been so loud as it was. Even so, sound Churchmen like the Archdeacon of Manchester, who knows the Theological Faculty of what (I fear) Mr. Stuart Jones would call the "dehumanised" University on the Irwell, is content to trust the Bishops to "save" the New Testament.[1]

The mere layman interposes here with a plea for commonsense. "It would be just as reasonable to urge that unless hydrodynamics are made a compulsory subject in Smalls, Oxford will produce engineers who are ignorant of that science."[2] But the Oxford don knows better. Brought up himself under the compulsion which he defends and fated to deal exclusively with compulsorily Hellenised students who, after long apprenticeship in Philistia, can be counted on not to learn anything unless they are obliged, he has a firm conviction of the natural depravity of man and an implicit faith in the Letter of the Law. "Surely education *is* irksome to the natural boy," says that ardent compulsionist Mr. John Murray of Christ Church.[3] It certainly is assumed to be so to the natural undergraduate. But it is a strange comment on the claim of "thoroughness"

[1] *Manchester Guardian*, November 22, 1911.
[2] *Globe*, November 16, 1911.
[3] *Manchester Guardian*, December 5, 1911.

and "character"-building which is put forward by others for the Grecian curriculum in schools.

The zeal of the defenders on this point leads them into some perilous admissions. It was argued, for example, by the President of St. John's, in a recent debate, that to relax Oxford's requirements is to root out the knowledge of Greek over large areas in this country. Yet, according to him and to other compulsory Grecians, it is the pride of Oxford, of Cambridge too, to have supplied the whole country, up to now, with an educated parish clergy; and by "educated" they mean Grecian. If this boast be true, where is the difficulty in finding everywhere, within a mile or two and for half a century to come, a missionary of Hellenism to put the would-be Grecian in the way he should go? If, on the other hand, it be not true—if the President of St. John's be right in his gloomy forecast—what has "compulsory Greek"—with "Divvers" thrown in—achieved for Hellenism in England in the last fifty years?

What these pessimists ignore, as is their habit, is the keen classical activity of the new and "dehumanised" universities. At Manchester, where there is a considerable class to learn Greek without Latin at all,[1] the students have just performed in Greek the *Choephoroi* of Æschylus[2]: only one of his plays has been staged at Oxford (and that was thirty years ago) and only one at Cambridge. At Cardiff, where there is another such class in addition to ordinary "Græco-Romans," they have acted already as many plays of Aristophanes as have been performed at either of the old universities. At Liverpool the classical students, with old students and their friends, meet regularly of an evening to read Greek plays and dialogues aloud, as students of German meet to read Goethe. Some Oxford colleges have Shakespeare Societies; but does one of them maintain a Sophocles Club? In my own experience, indeed, it is not because students in the new universities dislike Greek or have no use for it but simply because they are occupied most of their time in making good the deficiencies in the only kind of school training that they can obtain at present, that they omit the ordinary classical courses and drop away from the extra courses which it is always worth while to provide in Greek for non-Grecians. At Liverpool I was never without students of

[1] *Times* Literary Supplement, January 2, 1911.
[2] *Times*, March 14, 1911.

Greek who came in from the History School and the School of English Literature, not because Greek was required in these courses but because they could not get on without it.

A Natural History of Smalls

In the older universities too, as in the schools, we have to face the facts. And the central fact in Oxford is the examination called Responsions or colloquially "Smalls." The latter name dates from the first half of the last century when there were only two examinations in the B.A. course and these were "Smalls" and "Greats" or (as we should call them nowadays) an Intermediate and a Final Examination. The old Final Examination consisted of two collateral "Schools" or (as we should now say) subjects of study—Classics and Mathematics. The latter included Mechanics and in fact all the elements of what was then understood as Natural Philosophy. Classes were given in each and a very good candidate might thus obtain a "Double First." But about half a century ago, the old rule that a man must offer for his degree the non-classical as well as the classical subject broke down at Oxford through the incapacity of most college tutors to teach anything but classical scholarship and the older sorts of "mathematics." By the irony of events, the present *Censor Naturalis Philosophiæ* at Christ Church is Mr. John Murray. The crisis was embittered by the deep prejudice which some new kinds of natural philosophy provoked in minds not so much superstitious by nature as clerical by statute. This deliberate repulse of the New Learning which has shaped our time brought quick retribution in the shape of new "Arts" courses; in Classics without Mathematics of any kind; in Science without Philosophy; in History without Language and Literature; in Law without anything. Except in the Final Honour Schools of Theology and of Literæ Humaniores (under 200 students annually) and in the requirements for the undifferentiated Pass Degree (some 400 students more), the study of the Classics as an element in a liberal education disappeared from Oxford before 1870. The scruples of the religious were satisfied, as usual, by a mirage. They accepted and still tolerate "Divvers" as a certificate of Hellenistic Greek in chemists and biologists.

The disastrous mistake of including all degree courses in

Science, however specialised, within the old Faculty of Arts, was due mainly to snobbishness on both sides. The Arts men were confident that a degree of Bachelor of Science, being new, would be necessarily cheap and probably corrupt; an inveterate prejudice in Oxford which has cropped up again, this month, in an amendment proposed by the Council to the procedure for Research Degrees. The Science men were misled in sheltering their new studies under a title which however ancient and respectable was at the same time quite inappropriate. The Chemical course, for example, contained nothing but Chemistry and some elementary Physics; it was in no sense a course in Arts or even in an Art, except in so far as it included "Divvers" and was preceded like all other degree courses by Responsions. It was Science pure and simple and ought to have been labelled so.

Among all these changes, one thing alone remained, the requirement of Greek and Latin at the preliminary stage of "Smalls." Here, again, there is a widespread misconception to be corrected at the outset. It is not the fact, it never has been the fact and I sincerely hope it never may be, that the University of Oxford refuses to register a student who does not know Greek up to the standard of Responsions. Readers of *Verdant Green* will remember that it was normal in the 'fifties for an undergraduate to reside during several terms before going in for Responsions. Responsions, in fact, stood then where Pass Moderations stand now: and where the Intermediate stands at modern universities.

Here, as in so many other ways, blame has come to the university through the action of powerful corporations, whose boast it is, if we may believe their latter-day spokesmen, that they are organically unconnected with it. Residential colleges, beset with more applicants for admission than they had sets of rooms to offer, naturally selected for admission those who undertook to save college tutors some terms of drudgery, by passing Responsions before coming into residence. The practice is now almost universal; it has been extended already in special cases to our present Intermediate, the "First Public Examination," colloquially known as "Mods"; and it is certain that any one of the stronger colleges could play this game habitually, if it chose to do so. All it has to do is to present "this my pupil" to the Vice-Chancellor for admission to the

university and then "send him down" to his school or home till he is of standing for "Mods" and has passed. It was but a detail, that in time the university was forced to admit to Responsions itself—as well as to other examinations "in lieu thereof"—persons who were not yet registered members of the university at all but only "on the list" of some college. Even now, however, there are survivals: the Delegacy for Unattached Students, for example, does not insist that a man shall have passed Responsions before it admits him to residence; and a college occasionally grants the same privilege, especially to a man whose course involves him in what is known as an "additional subject." Thus Responsions, originally an internal examination designed to test an Oxford man's progress in his work and his fitness to enter on more advanced courses, all (it must be remembered) involving close study of Greek authorities, has been perverted to serve as an external obstacle-race by which to test the capacity for cram of certain science men, modern historians and students of modern languages; especially if they happen to have been educated at Greekless schools and so have not "climbed up some other way" with one or other of the numerous certificates of exemption.

Thus Responsions still tests Greek, Latin and Mathematics and nothing more, simply because, when Responsions was instituted, all the studies for the Arts degree were studies which required these subjects and nothing more, as the essential preliminary and equipment. History, Geography, General Knowledge and English were either to be acquired—as indeed they were—in the course of a man's Oxford reading or, in so far as they also were pre-requisites, it was the business of the college or hall or of an individual master to withhold an introduction to the Vice-Chancellor from any one who had not acquired them. The turning of "a piece of *Spectator* into Latin, . . . the construing of half a dozen lines of Livy and Homer and the answering of a few questions was a mere form. Mr. Slowcoach's long practice enabled him to see in a very few minutes if the freshman before him (however nervous he might be) had the usual average of abilities and was up to the business of lectures." Thus briefly does the author of *Verdant Green* describe his hero's "matriculation." All that the university cared about was that no one should waste its time or the time of any master who lectured publicly by attending advanced

courses at which his ignorance of Latin and Greek or of elementary mathematics would make him a nuisance. But it interposed no bar to any student who cared to come and see whether in time he could pass its Intermediate.

That, in brief, is the history and *raison d'être* of Responsions. It is one of the few survivals from an Oxford, not yet severely afflicted with the examination habit, which accepted a student without imposing any intellectual test at all, provided only that some master or society of masters was prepared to take the responsibility of supervising his work. It stipulated only that the candidate did not seek admission to its "final" courses before he had completed an "intermediate" course within the university itself.

It was an unfortunate accident—and I find no proof that it was anything more—that in the same generation in which unreformed Responsions became the standard of acceptance by a college, the university was remodelling all the rest of its curriculum and recognising subjects like Modern History and Natural Science, with which Greek stood in no necessary connexion at all. It was an accident too and a still more unfortunate one, that within the same generation the grammar schools reaped the retribution of their own incompetence, in the shape of that Greek-hating crop of parents which carried through the Industrial Revolution and invented the practice of Technical Education and the doctrine of Payment by Results. But the result was that, just when the colleges, hard hit by agricultural depression, were unable to extend any further their accommodation for students, a double stream of applicants began to flow from the old and from the new schools. It was fatally easy, therefore, for indolence, simulating prejudice and perhaps not untainted by it, to select a means of discrimination which has always penalised the Greekless school by mistake for the brainless boy; and is now perceived to be penalising the penniless boy by mistake for the Greekless school. "There is a large body of opinion outside the university which regards the maintenance of Greek as a social bar."[1] Or as an undergraduate put it: "I approve of compulsory Greek because it brings the right sort of men to Oxford."[2]

How delicate is the adjustment of the standard of Responsions to the residential capacity of the colleges is betrayed by

[1] *Times*, November 23, 1911. [2] *Times*, January 20, 1911.

instructive letters in the *Oxford Magazine* this term (February 15 and 29, March 14). Very moderate changes in the Greek part of the examination, which were rendered almost inevitable, even in Oxford, by recent public criticisms of "Smalls Greek," are already making it difficult to keep all available rooms filled with men who have passed or can count on easily passing "Smalls." One of the results . . . has been the transference to the sister university of men to the number [as complainant estimated] of some hundreds already. Others have been deterred from attempting to take a university course at all." At Cambridge, too, how completely successful the discrimination is between the Classical and the Modern-side boy was lately shown by Mr. Oscar Browning,[1] when he claimed that "abolition of compulsory Greek would not have increased our numbers [at the Cambridge Training College] by a single man" but added, all unaware of its significance, "I wish I could say the same of the insane regulations of the Board of Education, which compelled our students to pass before entrance a compulsory examination in history, geography and English literature." The candidates, it would seem, knew so much Latin that they could cram "Greek" without risk; but not all their Latin could secure them from failure in history, geography and English literature, the very subjects which would be their principal care as trained teachers and without discipline in which their own lives surely were but a sorry business.

The New Policy of the Compulsionists

"If at Oxford a man were asked whether he knew Greek and replied that he did and that the proof was that he had passed Responsions, the answer would be received with contempt." Only recently, in fact, the committee charged with ascertaining whether candidates for Research Degrees have received a "good general education" rejected an applicant who had passed not only Responsions but Pass Moderations as well. That is what Oxford itself appears to think already about the value of "Compulsory Greek." The facts are admitted by the Compulsionists themselves. As the President of Corpus himself wrote last November, "The minimum study at present required for

[1] *Westminster Gazette*, November 24, 1911.

admission to the university is not satisfactory either in method or result."[1] But he prefers what he described as "reform" to what he fallaciously calls "abolition." "One of our more urgent duties is to bring about such a reform, for the sake of many more of our students than those who are affected by the present proposal" to exempt mathematicians and science men.

The new policy is already before us in outline. I summarise letters by Mr. John Murray.[2] Greek, hitherto optional in the great majority of schools till the moment arrives for a boy to be crammed for Smalls, is to be a compulsory subject for a new Leaving Certificate to be awarded at seventeen by a Joint University Board. This Leaving Certificate will be "required of all candidates to the older universities"; but it will "entitle its holders" at the age of seventeen "to specialise forthwith to their hearts' content." Now the last vestige of excuse for Smalls is, as we have seen, that once upon a time it was a real test of proficiency in Greek and Latin, suitable to boys of nineteen who had already followed a real university course in them for some terms and intended to proceed to final courses which were also wholly in Greek and Latin literature, including all ancient history and philosophy, together with Mathematics no less liberally interpreted. It is only become the abuse that it is, because it has been misused by the colleges as a Leaving Certificate for boys under eighteen who have not yet come near the university and many of whom do not intend to open a Greek book again after passing. Responsions, in fact, already "entitles its holders to specialise forthwith to their hearts' content"; the only difference between the new compulsion and the old is that the new is to be applied at seventeen. Nothing that university examiners can do will force Greek back into the ordinary curriculum of the Greekless schools: all that they can effect under this monstrous proposal is to make Greekless boys cram Smalls Greek a year earlier than at present and to relieve the Grecians a year earlier from the necessity of pursuing the subject to its only profitable stage: for as Mr. Murray's programme most candidly warns us, "boys would ordinarily take the certificate some time prior to entering the university."

The incoherence of all this with the deliberate policy of

[1] Pamphlet, November 1911.

[2] *Westminster Gazette*, December 4, 1911; *Manchester Guardian*, December 19, 1911.

the great schools is obvious. Almost simultaneously comes an announcement made by the Head Masters of Eton, Charterhouse, Harrow, Rugby and Winchester "with the object of discouraging the premature study of Greek in preparatory schools," on the ground that such study "will cause inconvenience and waste of time." Boys will be admitted and classified on their knowledge of Latin, French and English; no Greek will be taught to boys of average ability until they are about fifteen. At Winchester the time thus saved "will be divided between history, geography, English and Latin"; and "boys who spend more than a year in that part [the Greekless part] of the school will never do any Greek"; and consequently will never have even the option of entering at seventeen for Mr. Murray's Leaving Certificate. Thus the Head Masters are discouraging preparatory schools from starting ordinary boys in Greek before they are fifteen; and the Compulsionists are proposing to make all Greek optional after the age of seventeen. In the two short years which remain, how much real Greek could even Dr. Rouse "preserve."

Meantime, even the Compulsionists are not likely to carry any "reforms" before the winter: and it may be well to look about us in the interval. Oxford may or may not do something decisive later on and Cambridge may or may not follow suit. In the meantime, where do the Greekless stand? In particular, are they really excluded from Oxford at all? If not, what is the way to get in? And who keeps the key?

Greekless Students already in Oxford

Two other facts, which have almost wholly escaped recent comment, deserve wider publicity, besides the quaint ancestry of Smalls which I have already detailed. One is that the exemption of special classes of students from the Greek in Smalls—about which all this trouble has arisen—and even from the Latin is, as a fact, nothing new. The facts were concealed by both sides in all the recent debates: they have now been published briefly by the Master of University College.[1]

But the special classes already so relieved are precisely those which to the lay mind would seem to be most urgently in need of Hellenisation, even if it had to be compulsory.

[1] *Times*, January 20, 1912.

Any native of Asia or of Egypt or of the Soudan, "not being of European or American parentage," may obtain permission to offer English as a substitute for either Latin or Greek and Sanskrit or Arabic or Persian or Pali or Classical Chinese for the other. Yet it is a chief claim of the compulsory Grecians that the civilisation, the "life," for which Oxford prepares men, is grounded in that of Greece and Rome and cannot be understood or practised without the "good general education" attested by Smalls Greek. But what becomes of this argument, when any genuine native of Asia or parts of Africa can be admitted to the Oxford degree courses in classics or chemistry or theology on mere proof of such acquaintance with elementary Sanskrit or Classical Chinese "as to test especially his knowledge of the grammar and language"? The formula of course is closely modelled on that which regulates Smalls Greek. In Smalls Pali the "set book" fills just 112 pages and is therefore commensurate with those prescribed in Greek. In English the "set books" are *The Merchant of Venice*, *Woodstock* and *The Talisman* and (for a wonder) "a knowledge of the subject-matter" is required, a refinement which does not appear in any other part of Responsions. The pedantry with which this exception is set out is only equalled by its futility. For exacting proficiency in Greek there was at least the pretext of an ancient (if obsolete) reality: and no one expects an institution so historical as Oxford to keep its requirements punctually up to date. But when did any one, in Asia or in Bedlam, learn Chemistry or study English Literature or even read Greats with the linguistic aid of Classical Chinese? What sweet effect on the character of a native of the Soudan, not being of British or American parentage, have 112 pages of Smalls Pali —or Arabic either—offered "to test especially his knowledge of the grammar and language"?

Existing Oxford Courses which require no Greek

The other unconsidered fact to which I have referred is that with one exception it is only for admission to courses leading to the B.A. degree that Oxford has ever required or appeared to require Compulsory Greek at all; and that the B.A. degree, though it was once the only formal certificate of proficiency outside the Professional Faculties, Theology, Medicine and Law,

has long ceased to be so. The one exception is Music and in Music, as in the other Arts, Smalls Greek is required; harmony and the sonata, like all human excellences, are rooted and grounded in Hellenism. But in the so-called "Research Degrees" in Letters and in Science the only initial qualification for a non-graduate, that is to say for a member of the general public aged twenty-one, is that he should "have given evidence that he has received a good general education satisfactory to the committee constituted for the purpose." There is nothing in the statute which regulates these degrees to limit the discretion of the committee in respect of remission from Greek; and by an amending statute the degree of any British university, however "dehumanized," admits without scrutiny. Nor does the constitution of the committee itself secure a majority of Grecians; the Faculty Boards of Theology and Literæ Humaniores have only one representative each in a total number of ten. What the practice of this committee may be it is perhaps best not to inquire too closely; but as we have seen already, it can be acquitted of the suspicion that it does not think for itself.

Besides the Research Degrees, there is now a whole series of "Diplomas" in Anthropology, Classical Archæology, Geography, Economics and Political Science, Education, Forestry and Rural Economy. In Classical Archæology both Greek and Latin are required, since the subject demands them: and the standard required is a high one. But for no other Diploma is Greek required. Candidates in Forestry and Rural Economy have to satisfy the same committee as admits to the Research Degrees; in all other courses the committee which provides the teaching and awards the Diploma is the sole judge of the candidate's "good general education."

Thus the university has already established a number of excellent courses for which Greek is not pre-requisite. All that is necessary, to give them the distinction and status which they deserve and to induce the university to add greatly to their variety, is for a sufficient number of the right sort of men —not, let me hasten to explain, in the conventional sense in which the words have been quoted elsewhere[1] but in their natural meaning—to come up and take them and having taken them to go out into the world and *succeed*.

These are the way into Oxford now, for the Greekless man.

[1] *Times*, January 20, 1911.

Who then holds the key? The one obstacle to the influx of the right sort of men for these courses is the reluctance of the colleges to take any student into residence who is not working at courses which "count," as the B.A. degree, with its Final Schools and Class Lists, "counts," in the inter-collegiate scramble. Accommodation for students in Oxford is after all not unlimited; the colleges and their third- and fourth-year men who "live out" take up nearly all that is available and of the unattached students a large majority are also reading for the B.A. If the colleges persist in giving the present preference to men who wish to take the B.A. course and consequently must pass Responsions, they are stultifying the action of the university when it established Research Degrees and Diploma courses alongside of it. Have they forgotten that even the oldest existing B.A. courses are an innovation barely a century old and that all except the Classical and the Mathematical Schools were created within living memory? Still more, have they forgotten that New College was ancient already when the first Oxford "Grecians" were harried for knowing Greek at all by the students of Tudor times; that Corpus, a Renaissance college itself, had for a while no "Grecian" on its staff but farmed out exceptional students to be taught Greek outside, as it would farm out Sanskrit now; and that a President of Corpus could be urged by his friend Linacre to learn just a little Greek for himself, with the assurance that it was really quite easy and the literature not dull? If the colleges have forgotten these things, it is time they were reminded that we live in a new Renaissance. If they remember but will not act, I fear we must class them after all with the undergraduate friend of the Master of University. "I approve," he said, "of compulsory Greek because it brings the right sort of man to Oxford."[1] There is a large body of opinion outside the university which regards the maintenance of compulsory Greek as a social bar. Is it possible that, for all their protestations, Oxford colleges are influenced by the fear that candidates for Diplomas and Research Degrees are not "the right sort of man"?

Is it however really the business of universities—even of residential universities—so to use their "power of the keys" as to "bang, bolt and bar the door" against aspirants to learning? *Aut disce aut discede* is as fair an offer for universities to make

[1] *Times*, January 20, 1911.

as for a great school, particularly if room be limited; but how are you to bid a wretched student "either learn or go" if you never allow him to *come*.

The Head Master of Shrewsbury has already spoken out in this sense.[1] "Is not the obvious solution that the university should leave the individual colleges to make their own terms for the admission of their own members? There can be no question that the standard would be safe in their hands and they would thus be enabled to secure the type of man who would most benefit by the education which they felt themselves best fitted to give." It is true that he is not so much concerned for Greekless scientists as for science-less Grecians: "I cannot believe that some college would not be found bold enough to exempt its classical and historical scholars from the thraldom of algebra and geometry." But his argument cuts both ways. His own dislike of "algebra" is the exact counterpart of a mathematician's dislike of "Greek": and both dislikes come from unacquaintance with the reality. Though he is mistaken in supposing that the university can permit, much less compel, on the main issue he may well be on the right lines. It is, in fact, the colleges, as I hope I have shown, which began by making Responsions do duty as their entrance examination; it is notorious that the stronger colleges have in effect discarded it already for higher "matriculations" of their own. On the other hand, the weaker colleges and their stalking-horse the "poor man" are already revealed as an obstacle to its reform, because a very small rise in the standard would force them to empty their rooms or revert to the ancient custom of letting men come up and stay up who had not passed Smalls.

It would be a pleasing demonstration of college autonomy—which is a fashionable doctrine in Oxford of late—if colleges would really vary their requirements a little in this way. Corpus, as a Renaissance college, might redeem its early Greeklessness by demanding "real Greek," to the standard, let us say, of Honour Moderations, before admitting to residence. Worcester might revert to a picturesque phase in its history and resume its mission of spreading Western culture among the modern Hellenes; an oral examination in Romaic would be the very thing; perhaps Dr. Rouse would oblige. Another college, discarding Greek, might substitute Welsh or Irish, "with special

[1] *Times*, December 29, 1910.

reference," of course, "to grammar and language." Balliol, which has long been kindly to the natives of Asia and Africa, has only to stay as it is; it already exacts, from Europeans, a higher standard of Greek and other things, than that of "Smalls." And if there really are any colleges which cater for the "idle rich," it would be a minimum of change for them to have no "matriculation" at all, like the Delegacy for Unattached Students.

It is easy to jest on such a topic; but the proposal only seems new and strange because it is immemorially old. It is in fact mere re-formulation of the pristine status of colleges in relation to the university. The colleges have accepted always, as they accept now, what students they like; it is only in question now, whether they like *the right sort of men.* The university welcomes all; gives the opportunities of learning to all; and tests the proficiency of all during their course and at its close. All that the university, as such, is concerned to do is to see that its own courses, within themselves, are coherent and efficient; that its masters are under no inducement or compulsion to lecture down to their audiences but only up to their subject; and that the financial interests of so-called "autonomous" societies are not allowed to debase, as they certainly do debase at present, the standards of attainment, whether in Greek or in Science, by which the university may think right, after reasonable trial, to reject all but "the right sort of men."

CONDITIONS OF CHEMICAL CHANGE

II. PHOTOCHEMICAL CHANGE IN GASES

PART I. HISTORICAL

By D. L. CHAPMAN, M.A.

In comparison with other branches of chemistry, photochemistry in its scientific aspect in the past has received little attention and is a science relatively in its infancy. The classical researches of Draper and of Bunsen and Roscoe and a few other interesting but isolated investigations which will be alluded to in this and a subsequent article almost complete the list of researches on a subject which promised from the outset to aid in an exceptional degree in the elucidation of the mechanism of chemical change. When it is remembered that the vast stores in plants of energy derived from the radiation of the sun are the result of the operation of photochemical changes, it can scarcely be doubted that a precise knowledge of the laws that control photochemical transformations would be of immense benefit to mankind. The comparative neglect of the subject in the past is therefore a matter of surprise.

The photochemical change which has been most extensively investigated is the interaction of chlorine and hydrogen. Change sets in when a mixture of these two gases is exposed to light and the hydrogen chloride that is produced dissolves with such ease in water that when the gases are confined in a glass vessel over water the decrease in volume is almost an exact measure of the amount of chemical change. At the ordinary temperature, in the dark, these two elements do not interact except in the presence of a catalyst such as platinum. That change takes place under the influence of light was first observed by William Cruickshank (Nicholson's *Journal*, 1801 (1), **5**, 202). In sunlight the action is so energetic that the mixture explodes (Gay Lussac and Thénard, *Mémoires d'Arceuil*, 1809, 2, 340). Cruickshank and later Dalton (*A New System of Chemical Philosophy*,

i. 300) noticed that when a mixture of hydrogen and chlorine is exposed to light diminution in volume does not take place immediately. There is no doubt that the cause of this phenomenon was an actual delay in the interaction of the gases: but neither of these authors attempted to prove that this was the case by a conclusive experiment.

The credit of having discovered that when a freshly prepared mixture of the gases is exposed to light the chemical change is more or less delayed belongs to Draper. He had previously constructed a delicate instrument called the tithonometer (*Phil. Mag.* 1843, **23**, 401) for measuring the rate at which hydrogen and chlorine interact and it was with this apparatus that his experiments were performed. Concerning the period of delay he says (*Phil. Mag.*, 1844, **25**, 9):

"A tithonometer exposed to the daylight is much too powerfully affected to allow the successive stages of change to be distinctly made out; the preliminary tithonisation is accomplished so rapidly, that the indications of it are merged and lost in the contraction which instantly follows. It is necessary, therefore, that we should operate with a *small* lamp flame.

"To such a flame I exposed a mixture of chlorine and hydrogen and marked the number of seconds which elapsed before contraction arising from the production of muriatic acid took place. The first indication of movement occurred at the close of 600 seconds.

"The index then moved through the first degree in 480 seconds,

"	"	"	second	"	165	"
"	"	"	third	"	130	"
"	"	"	fourth	"	95	"
"	"	"	fifth	"	93	"
"	"	"	sixth	"	93	"

and continued to move with regularity at the same rate. These observations, therefore, prove that a very large amount of radiant matter is absorbed before chemical combination takes place; and that in the case of chlorine and hydrogen the total action is divisible into two periods: the first during which a simple absorption is taking place without a chemical effect; the second during which absorption is attended with the production of muriatic acid." Later on he proceeds: "I tithonised the chlorine and hydrogen contained in the instrument and kept it in the dark for ten hours. On exposure to the lamp rays it moved after a few seconds, showing, therefore, that the change which had been impressed on the chlorine was not lost. In the former case 600 seconds had elapsed before any movement was visible."

The two experiments, taken in conjunction with one another, prove that the phenomenon is due neither to heating nor to delayed dissolution of the hydrogen chloride and can therefore only be occasioned by the non-formation of hydrogen chloride during the first part of the illumination. In a later paper by the same author (*Phil. Mag.* 1845, **26**, 473), a still more convincing experiment relating to the same point is described.

There are therefore two distinct stages to be considered, viz. the stage during which the gases from some unknown cause refuse to interact at their maximum rate, named by Bunsen and Roscoe the "period of induction"; and a subsequent stage during which interaction has been shown to take place in conformity with definite laws; this latter may be called the "normal period."

Draper in his first paper (*Phil. Mag.* 1843, **23**, 401) described observations he had made with the aid of the tithonometer as to the behaviour of the mixed gases during the normal period and showed that, in the first place, the rate of formation of hydrogen chloride, other conditions remaining the same, depended upon the composition of the mixture; the maximum sensitiveness was attained when the gases were present in equivalent proportions, a small excess of either being accompanied by a considerable diminution in the rate of chemical change. This peculiar effect was more closely investigated by Bunsen and Roscoe (*Trans. Roy. Soc.* 1857, **147**, 390), who found that the sensitiveness of a normal mixture is reduced by the presence of an additional 3 parts of hydrogen in 1,000 from 100 to 37·8 and by that of 10 parts of chlorine in 1,000 from 100 to 60·2. The same observers also demonstrated the remarkable catalytic effect of small quantities of oxygen, the presence of 5 parts of this gas in 1,000 reducing the rate of change from 100 to 9·7. It is here of interest to observe that it has been shown by A. Jodlbauer and H. V. Tappeiner (*Berichte der deutschen Chemischen Gesellschaft*, 1905, **37**, 2602) that oxygen exercises a remarkable retarding effect on the reduction of mercuric chloride by ammoniun oxalate, an interaction studied by Eder, which proceeds only in the light, in the manner represented by the following equation:

$$2HgCl_2 + C_2O_4(NH_4)_2 = Hg_2Cl_2 + 2CO_2 + 2NH_4Cl.$$

Occasion will arise later on to refer to the part played by this class of impurities during the induction period.

Draper (*loc. cit.*) also established the following facts concerning the "normal period" in the case of mixtures of constant composition:

(1) When the radiant source is constant, the amount of movement of the tithonometer is directly proportional to the times of exposure.

(2) The indications of the tithonometer are proportional to the quantity of incident rays.

In establishing the second of the above statements, it may be mentioned that the same source of light was used, definite fractions of the incident rays being cut out by obscuring sectors of a lens through which the light passed.

Both of the laws implied in these statements were afterwards submitted to a thorough investigation by Bunsen and Roscoe and established beyond doubt by accurate measurements.

The two laws amount to the statement that for a mixture of the same composition and for light of the same quality the chemical change is proportional to the energy absorbed.

Several investigations have been undertaken with the object of discovering a relation between the concentrations of the interacting gases and the velocity of change.[1] The results obtained in these researches, in the opinion of the writer, however, are vitiated by two circumstances: firstly, by the presence in the gases of impurities the effect of which at different stages of the interaction could not at the time be estimated; secondly, by the neglect to make an allowance for the decreasing amount of energy absorbed by the system as the chlorine diminished in concentration.

Several attempts have been made to correlate the velocity of chemical change with the refrangibility of the stimulating light. Seebeck in 1810 (Eder's *Geschichte der Photochemie und Photographie*, 1891, i. 93), by operating with coloured glasses, showed that the action was limited to the blue end of the spectrum. Berard (*Ann. Chim.* 1813, **85**, 309) believed that the maximum of action was in the violet. Draper (*Phil. Mag.* 1844, **25**, 5) experimented with the solar spectrum and states that in reality the indigo is the active ray and that the effect gradually diminishes towards each end of the spectrum from a maximum point which

[1] Of these researches that of M. Wildermann (*Phil. Trans.* 1902, **199**, 337) is the most extensive.

is in the indigo but towards the blue. The following table shows the activity assigned by him to the different rays:

Name of ray.	Force.	Name of ray.	Force.
Extreme red	—	Indigo	66·60
Red and orange	1·00	Indigo and violet	50·00
Yellow and green	1·90	Violet	44·40
Green and blue	25·00	Violet	20·00
Blue	42·90	Extreme violet	18·10

Too much importance, however, must not be attached to these numbers, since it is now known that the results obtained in this way would vary with the character of the sunlight at the time when the experiment was made, the mode adopted for dispersing the light and the length of the mixture of gases traversed by the rays, the maximum of action as the column of gas is lengthened appearing to move towards the end of the spectrum at which the rays are least absorbed by chlorine. Numbers of any precise scientific import concerning the influence of the character of the light are still wanting.

Various views relating to the mode in which the light brings about the change in the system have been expressed. These have for the most part been elaborated with a view to explaining the nature of the changes in the mixture during the induction period. Draper, as we have already stated, was the first to establish the reality of this phenomenon. He sought further, by direct experiment, to discover its cause. The facts as stated by him are these:

"When a mixture of chlorine and hydrogen is exposed in a tithonometer to the rays of an Argand lamp, with due precautions to ensure a correct result, four distinct periods of action may be traced:

"1st. For a brief space the mixture expands.

"2nd. For a much longer period it then remains wholly stationary, neither expanding nor contracting, though the rays are constantly falling on it and it is absorbing them.

"3rd. Contraction arising from the production and solution of muriatic acid begins, commencing at first slowly and then more and more rapidly.

"4th. And, after that contraction has fairly set in, it goes on with uniformity, equal quantities of muriatic acid being produced in equal times by the action of equal quantities of rays."

The change effected in the properties of the mixture,

according to Draper, was an increase in the affinity of chlorine to hydrogen acquired by the former element during its exposure to light. He describes an experiment which confirms this view. Two similar tubes were inverted over salt water and half filled with chlorine in the dark. One of these was exposed to sunlight, the other being left in the dark; an unequal volume of hydrogen was then added to both by lamplight. On placing the tubes side by side and exposing them to diffuse daylight, the liquid in the one containing the chlorine which had previously been exposed to light was observed to rise instantly, whereas the liquid in the other tube remained motionless for some time. The change in the chlorine is described as permanent or, at any rate, very lasting and quite unlike any transient effect due to a temporary elevation of temperature.[1] In the same paper several instructive experiments are described proving that rays of the same class are operative in bringing about the change in the properties of the chlorine and in causing the interaction of chlorine and hydrogen. In both cases, the maximum of action was found to be in the indigo space and to decrease gradually from that region towards both ends of the spectrum. The explanation offered for these facts is somewhat difficult to understand, as it rests upon an abstraction which was current at that time but has since fallen into disrepute. Solar light was regarded as composed of at least three imponderable agencies—light, heat and tithonicity or the power to bring about chemical changes. Each agency was alike dispersable by a prism and therefore the solar spectrum was in reality three superimposed spectra of the imponderable agencies. As Draper expresses it, during the pause in the first moments of illumination, the tithonicity is being absorbed and rendered latent by the chlorine, the electronegative properties of this gas being exalted at the same time, the chlorine the while acquiring a disposition to enter into combination with the hydrogen. But after the initial absorption of tithonic rays is over, equal quantities of the rays give rise to equal effects.

[1] We take this opportunity of correcting a mistake which has crept into the literature of this subject. It is erroneously supposed that Draper stated that solarised chlorine will combine with hydrogen in the dark. This, however, is not the case.

Bunsen and Roscoe (*Phil. Trans.* 1857, **147**, 881), on the other hand, regard chemical affinity as an inherent property of matter which is as unalterable as the mass of the atoms themselves. To speak, therefore, of the exaltation of the elective affinity of an element is, according to them, a badly chosen form of speech. The circumstance that chlorine and hydrogen can be intimately mixed without acting upon one another is to be explained by there being in that case a resistance which opposes the force of affinity. The removal of this resistance can be effected by various agencies, amongst which light must be included. They say: "The act by which resistance to combination is diminished and the combining power thus brought into greater activity, we call *chemical induction*; and we specify this as photo-chemical, thermo-chemical, electro-chemical and idio-chemical, according as light, heat, electricity or purely chemical action is the active agent concerned in overcoming the resistance."

When a freshly prepared mixture of chlorine and hydrogen is exposed to light, energy is expended in overcoming the resistance to combination; it is only after the induction has reached a definite stage that a visible formation of hydrogen chloride accompanies the illumination. The change therefore is only in the properties of the mixture. They were unable to confirm Draper's statement that chlorine which has been exposed to light and chlorine which has been made and kept in the dark differ in their behaviour towards hydrogen; they therefore discarded the hypothesis that the first action of the light was to produce an allotropic modification of chlorine. They further found that a mixture which had been illuminated sufficiently to carry it through its induction period returned slowly to its original condition if kept in the dark, *i.e.* the inductive effect was gradually and spontaneously lost.

An entire paper (*Phil. Trans.* 1857, **147**, 601) is devoted to answering a question of great theoretical significance, namely:

"In the combination effected by the light between the particles of chlorine and hydrogen, are the chemical rays expended in a relation proportional to the hydrochloric acid formed?"

Bunsen and Roscoe were led by a series of indirect measurements to conclude that a mixture of chlorine with an equal volume of an inert gas such as nitrogen did not absorb so much

light as a mixture of chlorine and hydrogen in equal proportions. The explanation which they offered was this: by virtue of its optical properties the chlorine in both cases absorbs the same amount of light, which is rapidly dissipated as heat; but when hydrogen is present, an additional amount of light is absorbed in bringing about the combination of chlorine and hydrogen. The additional quantity of light thus expended is the photochemical induction. It is the energy which overcomes the resistance to combination.

E. Pringsheim (*Weid. Ann. der Phy.* 1887, **32**, 384), whilst accepting Bunsen and Roscoe's result that a mixture of chlorine and hydrogen is more opaque than one composed of chlorine and an inert gas, criticises their hypothesis at some length on theoretical grounds and advances an explanation of his own. According to this observer, the first effect of the light is to decompose the molecules into atoms. The atoms, however, have only a very transitory existence apart and are rapidly re-associated in such a manner that they give rise to intermediate compounds. In the case of chlorine and hydrogen the intermediate compounds occupy the same volume as the original gas: in the dark, they slowly change back into the substances from which they were produced but when exposed to light they are transformed into hydrogen chloride. The preliminary dissociation being ignored, the successive changes are possibly embodied in the following equations:

$$\begin{array}{lccccc} 1. & H_2 + Cl_2 + H_2O = & Cl_2O & + & 2H_2 \\ \text{Vol.} & 1 \quad 1 \quad 1 & 1 & & 2 \end{array}$$

$$\begin{array}{lccccc} 2. & Cl_2O + & 2H_2 = & 2HCl & + & H_2O \\ \text{Vol.} & 1 \quad\quad 2 & 2 & & & 1 \end{array}$$

The rate at which the change expressed by the second equation takes place is of course regulated by the amount of chlorine monoxide present in the system, which again must depend on the duration of the previous exposure to light: consequently the formation of hydrogen chloride will be inappreciable during the first moments of illumination. Moreover, after the rate of formation and decomposition of the chlorine monoxide become equal, the quantity of hydrogen chloride produced in unit time will be proportional to the intensity of the chemical rays. To all appearance the hypothesis affords a simple and satisfactory explanation of the induction period.

It will also be perceived that for the first time water vapour is conceived as playing an important part in the change.

After Dixon (*B.A. Reports*, 1880, 593) had found that an explosion cannot be propagated through a dried mixture of carbon monoxide and oxygen, several attempts were made to discover whether moisture exercises a similar influence over the interaction of hydrogen and chlorine. The experiments of Pringsheim (*Weid. Ann. der Physik.* 1887, **32**, 384), Dixon and Harker (*Manchester Memoirs*, 1889 (iv.), 3, 118; 1890 (iv.) 4, 1), Baker (*Trans.* 1894, **65**, 611) and of Mellor and Russell (*Trans.* 1902, **81**, 1272) relating to this point are in fair accord: the result appears to prove that the presence of moisture accelerates the interaction of chlorine and hydrogen when it is brought about by the influence of light but is without effect on the explosive combination of the gases.[1]

Another striking peculiarity of a sensitive mixture of chlorine and hydrogen is noticed by Draper in his description of the tithonometer. When the mixture is exposed for an instant to light it suddenly expands and then contracts: the phenomenon is most strikingly seen under the action of an electric spark. Pringsheim (*loc. cit.*), who appears to have been unacquainted with Draper's work, found the expansion produced by a spark of given length to be independent of the state of induction of the gas and of the amount of hydrogen chloride that is formed; he therefore argued that it is not a consequence of heat disengaged by the union of the gases but is caused by the dissociation of the molecules into atoms. Bevan has questioned the accuracy of Pringsheim's observations and has brought forward experimental evidence, which in our opinion is conclusive, that the expansion in question is nothing but the thermal effect of a sudden formation of hydrogen chloride. The expansion was, in fact, found to be greater in the case of an insolated than in that of an uninsolated mixture and also roughly proportional to the rise in temperature and to the amount of hydrogen chloride formed.

The Draper or, as it is sometimes called, the Pringsheim effect, must not, as Pringsheim pointed out—and here there are no two opinions—be confused with a remarkable property, discovered by Budde (Pogg, 1871, 144, 213; Erg. 1873, vi. 477), which chlorine itself possesses. Budde observed that chlorine

[1] The rate of explosion in the dried mixture is a little faster than in the moist

expands when it is exposed to sunlight, the expansion being considerably greater than that observed with air in similar circumstances. The expansion, he found, is not due to the so-called heat rays. Believing that the effect was too large to be accounted for by a rise of temperature accompanying an absorption of luminous rays, Budde at first concluded that the molecules of chlorine were dissociated into atoms, a conclusion which he afterwards withdrew on finding that a rise of temperature in fair correspondence with the observed expansion actually occurred. Tyndall (P.A. **116**, 10) had previously attributed this behaviour of chlorine to the high absorption coefficient for the more refrangible rays and the low emission coefficient for the less refrangible rays of the element. Richardson (*Phil. Mag.* 1891, **32**, 277) has confirmed Budde's work and has also shown that the expansion is produced by the more refrangible rays of light, a result consistent with the view that the expansion is merely a temperature effect, as chlorine is known to absorb the rays at the violet end of the spectrum. Pringsheim has demonstrated that a single spark from a Leyden jar will not cause moist chlorine to expand. Budde's phenomenon can, in fact, only be perceived when a very intense light is used. The effect would not in itself be of much interest had it not been shown by several observers (Roscoe, *Watt's Dictionary of Chemistry*, 1875, **7**, 750; Baker, *B.A. Reports*, 1894, 493; Shenstone, *Chem. Soc. Trans.* 1897, **71**, 471) that it cannot be obtained with the dry gas. One of the simplest ways of accounting for the difference between the moist and dry gas is to suppose that the latter is transparent to those rays which are intercepted by the former. Cordin *Monatsh.* 1900, **21**, 660) maintains that this is the case. Mellor (*Chem. Soc. Trans.* 1902, **81**, 1289) has performed an experiment which at first sight appears conclusively in favour of the view that there is a marked difference in transparency by enclosing the insolation vessel of a Bunsen and Roscoe's actinometer within a large globe and filling this successfully with air, dry chlorine and moist chlorine. The large globe was painted in such a way that the insolation vessel could be illuminated with light which had filtered through 10 to 15 cm. of gas. When the globe contained air, the index moved eighteen divisions in a minute; using dry chlorine instead of air, the maximum reading recorded was two divisions in a

minute; but when moist chlorine was used, no action at all could be observed. Therefore, 10 to 15 cm. of moist chlorine will cut out all the chemical rays and the same length of the dry gas approximately eight-ninths. There is some difficulty in reconciling Mellor's observation with Bunsen and Roscoe's measurements. From the work of the latter authorities, the natural conclusion would be that moist chlorine should cut out an amount of light not differing greatly from that found by Mellor for the thoroughly dried gas. The results of experiments on these lines largely depend, however, on the character of the light used; hence it is unfortunate that Mellor does not mention the source of light used to illuminate the insolation vessel. It would appear, therefore, that there is still room for—in fact a pressing need for—experimental work connected with this important aspect of the subject.

Several hypotheses relating to the interaction of chlorine and hydrogen are based on the assumption that the change takes place through an intermediate compound. Mellor has shown that the intermediate compound cannot be chlorine monoxide or hypochlorous acid, by proving that neither of these substances will shorten the induction period; accordingly he assumes that if an intermediate compound be formed at all, it must be a complex molecule of the type $x\mathrm{Cl}_2, y\mathrm{H}_2\mathrm{O}, 2\mathrm{H}_2$, built up of molecules of hydrogen, chlorine and water. The hydrogen was introduced into the formula of the compound because it was supposed that chlorine by itself acquired no appreciable activity in sunlight.

Bevan, whose work was published almost simultaneously with that of Mellor (*Roy. Soc. Phil. Trans.*), made several interesting and important observations which led him to adopt an explanation somewhat similar to Mellor's but modified in one respect. Draper had found that a mixture of hydrogen with previously insolated chlorine instantly gave rise to hydrogen chloride on exposure to light. Bunsen and Roscoe denied that this was the case but Fremy and Becquerel (*Wurtz. Dict. de Chemie*, 1879, ii. 255) confirmed Draper's statement. Bevan found that previous exposure of the chlorine to light did, as a matter of fact, cause a difference but the increased power which moist chlorine thus acquired of acting on hydrogen is lost when it is passed through water. Bunsen and Roscoe's failure to confirm Draper's result was therefore due to their having

bubbled the gases through water into the insolation vessel after they had been exposed to light. It was this discovery, coupled with the fact that the least expansion capable of producing a cloud in moist chlorine is less in the illuminated than in the darkened gas, that caused Bevan to infer that the first action of light on a mixture of chlorine and hydrogen is to produce a soluble "addition" compound of chlorine and water and that the molecules of this compound can act as condensation nuclei for water vapour. The further action of light, according to Bevan, causes these molecules to combine with molecules of hydrogen and form a compound (similar to that postulated by Mellor) which finally breaks up into hydrogen chloride and water.

J. J. Thomson (*Proc. Camb. Phil. Soc.* 1900–1, **11**, 90) has proved with the aid of an electroscope of the C. T. R. Wilson type that either no free ions are formed in a mixture of hydrogen and chlorine when it is exposed to light or that, at any rate, the number over and above those contained in the darkened gas is negligible. A very slow interaction of chlorine and hydrogen takes place in the presence of those radium rays which will penetrate thin layers of glass and aluminium (W. P. Jorissen and W. E. Ringer, *Berichte der deutsch. Chem. Gesell.* 1905, **38**, 899). There is therefore very little experimental evidence favouring the view that the interaction is necessarily preceded by dissociation.

The theories of photochemical induction existing at the time that Burgess and the writer began to work on the subject can be arranged under three heads, namely :

(1) *Draper's theory :* That the light increases in some unknown way the electro-negative properties of the chlorine and thereby exalts the affinity between chlorine and hydrogen.

(2) *Bunsen and Roscoe's theory :* That the light gradually removes the resistance which opposes the force of affinity.

(3) *The intermediate compound theories :* That chlorine and hydrogen cannot interact directly but that the change must be considered as consisting of several distinct chemical transformations.

It will be seen that both Draper and Bunsen and Roscoe approached the problem in a way essentially different from that pursued by the later investigators : they regarded the induction period as something essentially new and inexplicable by known

chemical laws; they regarded it moreover as a phenomenon capable of pointing the way to broader generalisations and sounder views on the nature of chemical change. But if the existence of an intermediate compound be postulated, the period of chemical induction is merely a particular manifestation of known laws.

In a subsequent article, recent work dealing both with the problems raised by the researches of the earlier investigators and with the new issues which have arisen in the investigation of these will be described; the present position of the subject will then be reviewed.

THEORIES AND PROBLEMS OF CANCER

PART I

By CHARLES WALKER, D.Sc.

WHATEVER may have been the case in the past, as far as our knowledge goes, at the present time, living matter exists only in the form of small masses of a complex jelly-like substance known as protoplasm; these masses of protoplasm are called cells. As a rule, the cells are so small as to be quite invisible to the naked eye; under the microscope, however, they are seen to be composed of certain definite parts.

Living organisms, including both plants and animals, for convenience, may be divided into two great groups, the unicellular—in which the individual contains but a single cell; and the multicellular—in which the body of the individual is composed of a number of cells. Both unicellular and multicellular organisms react to external stimuli. The unicellular animal will seize particles of food, will swim away if disturbed and may form for itself a covering if the particular puddle of water in which it lives be dried up; a plant will grow towards the light; a very definite response is given by a man if some one tread upon his toe.

It is obvious that in the case of unicellular organisms the reactions take place entirely within the single cell and that in order to produce an effect upon a number of separate cells the stimulus must be applied to each individually. The way in which multicellular organisms react to stimuli is very different. All the cells composing the body of the animal or plant may respond to a stimulus applied to only a few; on the other hand, the stimulation of one group of cells may be followed by a reaction in an entirely different group. A large proportion of the body of a man responds to a sudden and violent pressure upon the toe; saliva drips from the mouth of the hungry dog when he sees food.

It is obvious, therefore, that in multicellular organisms there is some interaction between the cells and groups of cells which

are included in the body of the individual. Different groups of cells are influenced by each other, the whole body being affected by stimuli applied only to a part and what happens in the cells of one part of the body may depend upon or may influence what happens in the cells of other parts. The phenomena dependent upon this correlation between the parts of the body of the multicellular organism may all be included under the term co-ordination.

Some theories which profess to explain the nature of co-ordination involve the acceptance of a metaphysical power or influence; I refer particularly to Driesch's "Entelechy"—a principle he postulates as controlling development, regeneration and the interaction between the parts of living organisms. If a sufficiency of physical facts be lacking to afford a satisfactory explanation for a particular phenomenon, it may—or may not—be reasonable to postulate some metaphysical agency to account for it; whether it be not safer to leave the phenomenon in part unexplained in the hope of further evidence being discovered is, to some extent, a matter of personal opinion; if, however, an explanation, which satisfies the requirements of the case, be forthcoming which is based entirely upon observed facts, the introduction of any metaphysical assumption would appear to be quite unnecessary and undesirable from every point of view.

In the explanation of the particular phenomena of co-ordination dealt with here, I think it will be found that no recourse to powers or influences which are beyond proof or beyond our understanding is in any way necessary.

Among the higher animals what is perhaps most generally understood by co-ordination is a consequence of the existence of a nervous system. Returning to the example of a man whose toe is trodden upon, the reaction takes place because the stimulus applied to the toe is conveyed to the brain through the nerves; the brain in response sends impulses through other nerves both to the stimulated part and to other parts of the body, producing a number of movements which include many more parts of the body than that to which the stimulus was applied. These responses to stimuli are dependent upon the continuity of the nervous system: if the nerves of any part of the body in man or one of the higher animals be separated from the brain, the usual sequence of events following a stimulus is interrupted—the movements, in fact, are said to

be inco-ordinate. In spite, however, of the fact that a breach in the continuity of the nerves—the connexions between the brain and the various parts of the body—involves a disturbance or complete cessation of the usual responses to a stimulus applied to a part of the body, still, so long as the cells are alive, some measure of response is to be observed whenever a stimulus is applied. If, for instance, a man sustain such an injury that the nervous connexion between his brain and legs be cut off completely, he will draw up his legs if the soles of his feet be tickled or pricked, though he will be completely unconscious of the stimulus and of the reaction. It is thus evident that co-ordination of parts still exists in a region of the body which has no nervous connexion with the brain, though it be of a lower and less complete kind.

If we go lower in the animal kingdom, we find that forms of co-ordination continue in the body of the organism after the destruction of parts of the brain which if effected in man would have destroyed the same forms of co-ordination. In a frog, for instance, the destruction of parts of the brain which would cause immediate death in man leaves a frog with almost a full complement of co-ordinate responses to stimuli.

If a muscle with the nerve supplying it be removed from the leg of a frog and the nerve be stimulated, the muscle contracts; moreover, the particular kind of contraction produced depends upon the kind of stimulus applied. Here we have a lower form of co-ordination but still co-ordination of the same order.

In the lowest forms of multicellular animals a nervous system does not exist but we find an insensible gradation in co-ordinate phenomena between the most complex observed in man and those observed in animals in which there is no nervous system. Co-ordination, in fact, obviously exists quite independently of a nervous system. Plants will grow towards the light and react as whole individuals to various other stimuli. Much more striking examples may be found in particular plants. In *Drosera*, for example, the leaves are provided with numerous tentacles having glandular enlargements at their ends. The tentacles around the margin of the leaf are long, those in the middle are short. The glands secrete a quantity of a glistening and sticky substance which insects mistake for dew or honey and are attracted. If one of the short tentacles be stimulated, all the long peripheral tentacles bend over so that their enlarged

ends cover the middle of the leaf; if an individual long tentacle be stimulated, only that particular tentacle moves. If an insect touch one of the central tentacles, it is held by the sticky substance and all the long tentacles bend over and enclose it; if an insect touch one of the long tentacles, it is also held fast by the secretion and the tentacle bends over and deposits the victim in the centre of the leaf, whereupon the central tentacles are stimulated and so all the other peripheral tentacles bend over and enclose the insect. When the prey has been secured, the glands proceed to secrete a ferment that digests the insect and the products are absorbed by the leaf. This is a far more complicated phenomenon than the turning to the light by parts of a plant but both are apparently the reaction either of the individual as a whole or of parts of the individual to stimulus. It is difficult to avoid the conclusion that, as there is an almost insensible gradation between such phenomena as these and the co-ordinate actions of man, all are connected in some way and are derived primarily from some common potentiality inherent in living matter when it is in the form of multicellular organisms.

When we pass from the multicellular to the unicellular forms, we realise that the cells of the multicellular organism possess a power quite different from any possessed by the unicellular forms, that of reacting in concert. Here again we find no real gap, colonial forms of unicellular animals providing the necessary links. In some unicellular animals, the individuals are attached to the ends of the branches of a common stalk; each individual is really a separate and independent organism and if separated from the stalk is in no way incommoded but proceeds to form a new colony. In spite of this, if anything disturb an individual which is still attached to the common stalk, the individual contracts, its particular twig of the stalk contracts, the main stalk contracts, the other individuals contract and a reaction in the whole colony of unicellular animals takes place though a stimulus has been applied to only one or two.

In the phenomena resulting from the application of a stimulus to living matter, we have then a regular and imperceptible gradation between the simplest—those observed in unicellular forms—and the most complex—those observed in the higher animals, including man.

The phenomena we have considered hitherto have all been

due to reaction to obvious stimuli, whether a nervous system be present or not. There is however a manifestation of co-ordination which has met with comparatively little attention which is not connected with reaction to obvious stimuli; it would appear to be an inherent characteristic of living matter when in the form of multicellular organisms and is far more subtle than the phenomena already mentioned.

Multicellular organisms commence their existence as single cells.[1] In the majority of cases these single cells are formed by the fusion of two cells—generally derived from separate individuals—which after fusion into single cells are known as fertilised ova. The fertilised ovum divides into two cells, each of these divides again and this process of division goes on until the whole organism is built up. In the early stages the cells produced from the division of the fertilised ovum are known as blastomeres. If a fertilised ovum of Triton, Amphioxus or certain other animals be taken at a stage when it has segmented into, say, four blastomeres and the blastomeres be shaken apart, each of the separate blastomeres—which we will for the sake of convenience designate as A, B, C and D—will develop into an embryo proportionately smaller than the embryo produced from the whole ovum but complete in all its parts.[2] What is the significance of this fact? It means that each blastomere is capable of producing all the tissues of the body—that the cells derived from each give rise to all the various types that are found in the organism. But it means much more than this. If the blastomeres be left together and the ovum be allowed to develop in the ordinary manner, the cells descended from A do not include all the types of cells that are produced by B, C and D. It follows that A, which has the potentiality of producing all that in the ordinary course of events would be produced by B, C and D, does not do so when it forms part of a multicellular individual. It proves that there is a common influence dominating the cells forming a multicellular individual which causes them to be in some respects interdependent and, under normal conditions, limits the lines along which differentia-

[1] The cases in which new individuals are produced from buds or cuttings do not affect the general argument here advanced.

[2] Driesch, H., *Analytische Theorie der Organischen Entwicklung*, Leipzig, 1894; Morgan, T. H., *Anat. Anz.* x. 19, 1895; Wilson, E. B., *Arch. f. Entwick.* iii. 1, 1896; and many others.

tion and multiplication into groups will take place. I have called this form of co-ordination "somatic"[1] and the phenomenon seems readily explicable as the result of natural selection upon variations. Variations occur in all directions and only where variation towards interdependence occurred in contiguous unicellular organisms could multicellular organisms have been produced. There is abundant evidence to prove that the general potentiality shown by the separated blastomeres is retained to a certain extent by the cells forming the bodies of adult animals, though it becomes less as the later stages of evolution are reached and is probably at the minimum in mammals.

It is quite clear that if a portion of the body of an animal be removed, the cells of that part cease to be in somatic or any other kind of co-ordination with the body from which it is separated. There is, however, a process which is perfectly normal and part of the healthy life history of every multicellular organism by which some of the cells pass out of somatic co-ordination.

We have seen that a new multicellular individual is produced by the fusion of two cells derived from two pre-existing individuals of the same kind, the parent organisms. These cells which are separated from the body of the multicellular organism are known as "gametes"—the ova or eggs in the female, the sperms in the male. It is quite evident that the gametes themselves pass out of somatic co-ordination, as they separate from the body in which they arise; but it is also certain that the cell ancestors of the gametes are out of somatic co-ordination during many generations with the body which produces them. That these cells are in fact practically independent organisms living in a parasitic manner upon the parent body is very obvious in certain plants in which the gametogenic cells (those from which gametes will be produced after a number of cell generations) actually destroy and live upon adjacent cells. We have then the established fact that certain cells derived from the bodies of plants and animals, including man, pass out of somatic co-ordination by a purely physiological (normal and healthy) process, though they remain attached to or contained within the body for some time, living upon it in a parasitic manner.

[1] Walker, Charles, *Essentials of Cytology*, Constable, London, 1907, and elsewhere. Soma is the term generally applied to the body of the multicellular organism, excluding the reproductive cells.

The idea that the cells of malignant growths have escaped from control and live in a parasitic manner upon the parent organism forms the essential basis of most of those theories as to the nature and origin of cancer that have met with any very general acceptance. Therefore, though the hypothesis that the cells of cancer have passed out of somatic co-ordination was, I believe, first formulated by me in the particular sense suggested here,[1] I can claim but little originality with regard to the idea. Many explanations both before and since have been based upon ideas fundamentally similar in meaning, though they vary more or less in mode of expression and are influenced by the special points of view and the particular facts emphasised by the person who enunciates them, the latest of this group of hypotheses being that of the late Sir Henry Butlin[2], who developed the idea to the further stage of describing the cancer cell as a parasitic protozoon (unicellular animal) derived from the tissue cells of the host and even went so far as to give the various forms it adopts generic and specific names.

James Ewing[3] gives a very excellent account of the development of this class of hypothesis from Cohnheim's theory of embryonic inclusions. He shows how the theory of "Cell Autonomy" has grown gradually and regularly from this origin. By "Cell Autonomy" he implies something very similar to what I had already called "Somatic Co-ordination."

It is necessary here to consider the capacity to undergo differentiation which exists in the cells of the fully formed multicellular organism.

We have already seen that the bodies of multicellular animals are built up from the cells derived from a single cell, the fertilised ovum. The earlier generations of cells are all very much alike; it is only when the development of the embryo has advanced to a considerable extent that the different groups of cells destined to form the various tissues of the body begin to differ in shape, size and other characters. When the body is completely formed, it contains within it cells which differ from each other to an extraordinary degree, bearing but little resemblance either to each other or to the ovum from which they were derived. Now,

[1] *Essentials of Cytology* (1907) and elsewhere.

[2] "Two Lectures on Unicellula Cancri," *Brit. Med. Journ.* Nov. 25, Dec. 2, 1911.

[3] "Cancer Problems," *Arch. Internal Medicine*, vol. i. 1908.

in the ordinary course of events, after differentiation has taken place, cells tend to produce like cells and do not produce any other type. In the higher animals, after differentiation has taken place, the cells are incapable of producing any cells but those similar to themselves. The formation of scar tissue after an injury is an exception in a sense but one that does not affect the general rule. As we go lower in the animal kingdom, we find that the cells that have become highly differentiated possess more and more of the power to produce cells which are unlike themselves. A remarkable instance of this is afforded by some experiments upon the eyes of salamanders and newts. The earlier generations of cells produced from the fertilised ovum are arranged in two layers, the epiblastic and the hypoblastic; later on a third group is produced between these two layers, the mesoblastic cells. Now the crystalline lens of the eye is produced from an ingrowth of epiblastic cells which are very highly differentiated, being flattened and transparent and quite unlike any cells produced from the mesoblast. But if the lens be removed from the eye of the salamander or newt, a new lens is produced from mesoblastic and not from epiblastic cells.[1] In mammals, however, such a power does not exist, some groups of cells, *e.g.* those forming the skin, being capable of multiplying and producing other differentiated cells like themselves but never any cell unlike themselves. As we approach the lowest multicellular animals, we find this general potentiality retained in greater force and the whole body may be reproduced from a half or even smaller part, as in Planaria or Hydra. In plants, even in the later stages of evolution, this potentiality is often retained in a high degree, so that a whole individual may be produced by a simple process of growth from a small group of already fully differentiated cells which have been forcibly separated from the parent organism. It would appear that, though somatic co-ordination still exists, in all these cases where a high degree of general potentiality is retained by the differentiated cells, its sway has not become so absolute as it is in the case of vertebrates. There is in fact a regular gradation from cases in which a small specialised portion of an organism is capable, when cut off, of continuing a separate existence and developing all the parts of the parent organism, to cases in

[1] Wolff, Gustav, *Archiv f. Entwick.* i. 3, 1895; Müller, E., *Archiv f. mikr. Anat.* xlvii. 1, 1896.

which a part separated off dies at once and the power of regenerating specialised cells has almost disappeared.

Now cancer appears to occur only among the vertebrates, in which group of organisms somatic co-ordination is more perfect than in any of the organisms from which they are generally supposed to have been evolved[1] and in which the general potentiality of the cells for regeneration reaches its lowest ebb. In these organisms it would appear that when the cells pass out of somatic co-ordination while still remaining connected with the body in which they occur, excluding of course the reproductive cells which are thrown off, they continue to multiply and behave to some extent as independent organisms, though they no longer possess the capacity of undergoing differentiation necessary to produce a new individual similar to the organism from which they arose. In plants—in which, apparently, cells may be thrown out of somatic co-ordination by artificial stimuli (apospory and apogamy)—the cells proceed to form new and complete individuals. It seems then probable that the behaviour of the cells forming malignant growths after having passed out of somatic co-ordination is due to a diminution of the general power of undergoing differentiation in the progeny of the specialised tissue cells of the vertebrate body and the increase in the general influence of somatic co-ordination in these animals, which is perhaps partly due to the presence of their particular form of nervous system.

Personally I have great doubts as to whether cancer really occurs in any animals other than mammals. Several instances in which numbers of fish were stated to have developed cancer in particular fish-cultural establishments have proved to have been growths due to parasites. The extraordinary similarity in microscopical appearance—particularly in the case of the ordinary pathological preparation—between cancer and certain growths produced by parasites throws great suspicion upon the other cases described as occurring in fish and upon the few described as occurring in amphibia. The so-called cancers in fish and amphibia have not been sufficiently investigated and if the existing observations have been misinterpreted, the theories advanced here will be strengthened rather than weakened, as among mammals somatic co-ordination appears most perfectly

[1] Arthropods, etc., are excluded from this theory as probably not being in the direct line of evolution of the vertebrates.

developed and the power of undergoing differentiation least perfectly.

There are many observations which support this theory; we may, I think, dispense with the details of the clinical facts that bear upon it—they are so well known that it is not necessary to do more than point out the general behaviour of malignant growths. In the earlier stages they cause no symptoms at all; the symptoms are always secondary and due to pressure upon or invasion of some part of the body. Pain exists only as a response to this pressure or invasion. The growth of the tumour is more marked than the destruction of already existing tissues and the destruction is probably entirely mechanical, that is to say, it is due to such causes as pressure and the obstruction of the blood supply brought about by the mere presence of the tumour cells, not by any intrinsic destructive properties of the cells themselves. Clinically, in fact, the cells of a malignant growth act as parasites.

There are several other facts of a general nature which point to the same conclusion. Malignant tumours never have any nerve supply. Though in the rare primary tumours occurring in nerve sheaths a nerve may be involved at an early stage, there are no nerves belonging to the tumour. This further accentuates the suggestion that the controlling influence of the nervous system as a whole in vertebrates modifies and influences in some way the somatic co-ordination. Malignant growths are common only in those tissues in which cell proliferation takes place normally throughout life and are frequent in a tissue in proportion to the amount of proliferation usual in the cells forming it. Cancer is generally of more rapid growth in young persons than in old. The age at which cancer most commonly occurs is that at which physiological cell proliferation is becoming less active and therefore cell divisions produced by some unusual stimulus must involve a greater departure from the normal course of events than is the case in younger tissues. Cancer apparently cannot occur except in tissues in which multiplication of the cells is taking place. After the human embryo is fully formed, the nerve cells never proliferate and malignant growths originating in nerve cells are unknown.

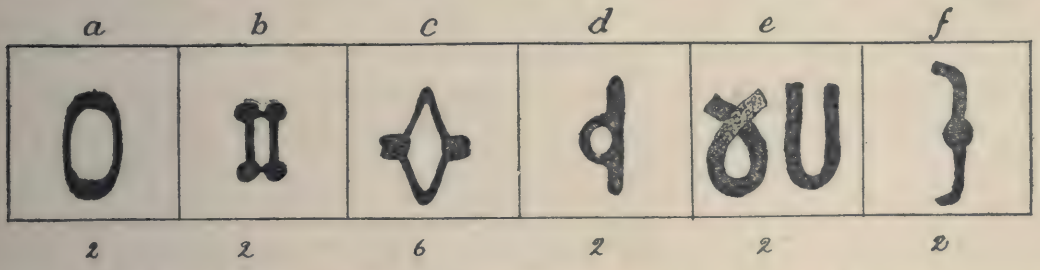
As we have already seen, the body of a multicellular organism originates from a single cell, the fertilised ovum. The

division of the ovum into two daughter cells and the divisions in the subsequent generations of cells thus produced is a complicated process. A cell, as has already been pointed out, is a small mass of protoplasm but it is not a simple homogeneous mass of this substance. Every cell contains within it an area, generally surrounded by a membrane, which is denser and differs in other respects from the rest of the cell. This is the nucleus. The remainder of the protoplasm forming the cell is the cytoplasm. When a cell is about to divide, radiations appear round two centres in the cytoplasm and as the time of actual division approaches these two centres separate more and more widely, the radiations forming a spindle of fibres between them. The whole figure presents an appearance similar to that exhibited by iron filings when spread upon a piece of paper laid over the poles of a magnet, except of course that the figure in the cell is three-dimensional.

While this has been taking place, certain bodies, chromosomes, have formed within the nucleus. These bodies contain a substance present in the nucleus in a scattered condition at other times which collects, during the process of division, to form the chromosomes; this substance is called chromatin, on account of the readiness with which it takes up the dyes used in making microscopic preparations. During all the divisions which take place in the building up and keeping in repair of the bodies of animals and plants, the chromosomes are similar in appearance, their form being that of rods, U's or V's. They are constant in number in the cells of individual species of animals and plants. Thus in the cells of man thirty-two chromosomes appear upon division, in the newt twenty-four, in *Ascaris megalocepha launivalens* (a parasitic worm in the horse) two; and so on. When the chromosomes are fully formed, the nuclear membrane disappears and each of the chromosomes becomes attached to one of the fibres of the spindle which has been described already and lie flat upon the equatorial plane; the whole body of the cell acquires an hour-glass shape and the chromosomes split longitudinally, a half of each travelling towards opposite poles of the spindle. As these half chromosomes collect around the respective poles, the body of the cell divides into two at the constricted point in the middle, producing two daughter cells, each of which contains an exact representative half of each of the chromosomes present in the parent

cell. New nuclei are formed from the chromosomes collected together at what were the poles of the spindle; when division takes place in the daughter cells, precisely the same process is gone through again.

Now it has already been pointed out that the fertilised ovum is produced by the fusion of two cells, generally from two individuals of opposite sexes. How then are we to account for the fact that the number of chromosomes is not doubled upon the production of a new individual? Some time before those cells are produced which are actually separated from the body in the form of sexual cells or gametes, a mode of division takes place which differs in important respects from the ordinary or somatic[1] mode of division; in this form, which has been called "meiotic" division,[2] only half the number of chromosomes usually found in the cells of the species appears. Actually



each of the chromosomes represents two of the chromosomes which appear in the ordinary or somatic form of division. Instead of appearing in the form of rods, U's or V's, the chromosomes appear in certain peculiar forms constant in character in each species. In species that have been particularly investigated a definite number of shapes and a definite number of each of these shapes is found in each cell as it passes through the meiotic phase.[3] The shapes and the number of chromosomes of each shape in man are indicated in the accompanying figure. When these chromosomes become attached to the spindle fibres, they lie with their long axes parallel to the spindle, that is to say, at right angles to the position taken up by the chromosomes in a somatic division. When they divide,

[1] Appertaining to the soma or body in contradistinction to the reproductive (gametogenic) cells.

[2] Farmer and Moore, *Quart. Journ. Micro. Science*, vol. xlviii. Part 4, 1905.

[3] Baumgartner, *Biol. Bull.* 1904, vol. viii. p. 1. Moore and Arnold, *Proc. Roy. Soc. London*, 1906, vol. lxxvii. B. p. 563. The Meiotic Phase includes the whole series of phenomena in which the chromosomes are reduced in number to one-half of that found in the somatic (body) cells.

instead of splitting longitudinally, they divide transversely across the middle. When the daughter cells are formed, it is obvious that, as only half the number of chromosones appear in this particular form of division, each daughter cell will contain only half the number of chromosomes usually found in the cells of the species of animal or plant from which it is derived; therefore the subsequent generations of cells, up to and including the mature gametes or sexual elements, will also contain only a half number of chromosomes. Thus when two gametes fuse in the fertilisation of the ovum, the somatic number of chromosomes is restored and appears in the cells of the new individual which is produced from it.

It has already been pointed out that the only cells included in the body of the multicellular organism which normally pass out of somatic co-ordination as part of the physiological process are the gametogenic cells—those of which the descendants are destined to be separated from the body as gametes. In the gametogenic cells which have passed through the meiotic phase the number of chromosomes is reduced to half the usual (somatic) number. Now the Meiotic Phase is noticeable also among the cells of cancerous growths [1] and the observation of this phenomenon has a very direct bearing upon the view that the unique characters of cancer are due to the fact that the cells have passed out of somatic co-ordination. From the outset, the observations recording the occurrence of the meiotic form of division among cancer cells have been entirely misunderstood by many writers and have consequently been subject to considerable misinterpretation. The characteristic feature of the meiotic phase is the symmetrical reduction in the number of the chromosomes to one-half of that found in the somatic cells. When this form of division was first observed, before its real significance was understood, the term "heterotype" was applied to it. Many different kinds of abnormal conditions may influence the process of division in ordinary somatic or body cells, causing them to appear unlike those which take place usually. The chromosomes may be distributed asymmetrically among the daughter cells, the consequence being that cells will produce with a greater or less number than is normally observed. The chromosomes may also be crowded or clumped together in the form of

[1] Farmer, Moore and Walker, *Proc. Roy. Soc.* vol. lxxii. 1903; and B, vol. lxxvii. 1906; Walker and Whittingham, *Journ. Pathol. and Bact.* vol. xvi. 1911

irregular masses; the irregularities may occur among the dividing cells of any tissue and unfortunately many writers have applied the term "heterotype" to all of them as well as to the meiotic or true reduction division which occurs normally in the gametogenic cells. While some writers have deliberately applied the term "heterotype" to any division which was unlike the somatic form of division, others of less experience have actually mistaken abnormal (pathological) cell divisions for the true meiotic form and a great deal of confusion has arisen. Thus Calkins says[1]: "The observations of Farmer, Moore and Walker on heterotypical mitosis[2] in cancer cells indicate only evidences of the degenerative changes which the majority of cancer cells must undergo. . . . Cytologists, furthermore, are constantly demonstrating that heterotypical mitosis is only a condition which may be assumed by cells under abnormal treatment. Haecker, for example, has shown that normal somatic division figures are transformed into heterotypical mitoses by treatment with ether and other poisons and Miss Bonnevie has recently shown that heterotypical mitoses are common enough in normally developing cells of various animals and plants." It was known long before Haecker took up the subject that various poisons produced irregular and deformed cell divisions[3] and, as has been recently shown,[4] these are probably characteristic of any inflammatory process. Inflammation, it must be realised, follows upon any kind of injury or irritation. It may result from a mechanical injury or irritant, from poisons or irritants introduced as such among the cells, also from poisons or irritants produced among or within the tissue cells by micro-organisms which have gained access to them. A very slight departure from the normal conditions is apparently sufficient to produce abnormalities in the process of cell division. The complication of the phenomena due to the cells of cancer having passed out of somatic co-ordination by the addition of phenomena due to inflammatory processes may very likely have led to confusion in the case of some observations.[5]

The main evidence that meiotic phenomena occur in cancer

[1] *Journ. Exper. Medicine*, vol. x. No. 3, April 1908.

[2] The technical term for cell-division.

[3] Galeotti, *Beitr. z. path. Anat. u. z. Alleg. Path.* Jena, xiv. s. 288, 1893, and others.

[4] Walker and Whittingham, *Journ. Path. and Bact.* vol. xvi. 1911.

[5] Walker and Whittingham, *op. cit.*

cells consists in the comparison between the records of the numbers of chromosomes found in individual cells of cancer, reproductive tissue and inflammatory tissue respectively. The numbers of cells dealt with are so large as to render the influence of small personal errors or of chance of no practical importance.

The accompanying figures[1] show graphically how the processes of reduction in the number of chromosomes in the cells of (1) human reproductive tissue, (2) a rapidly growing but unulcerated (uninflamed) cancer, (3) a cancer with a considerable amount of inflammation superimposed on the meiotic phenomena and (4) ordinary inflammatory tissue, compare with each other. A graph including 800 cells from inflamed and uninflamed malignant growths confirms the points illustrated here (5).

Other evidence shows that the peculiar forms of chromosomes found usually only in gametogenic cells appear also in the cells of malignant growths but not in those of inflammatory tissue.[2]

Against this evidence are only generalised statements that the numbers of chromosomes in the cells of malignant growths do not appear to be exactly half the somatic number and this in spite of the fact that over five years have elapsed since the first set of figures were published.[3] No systematic counts supporting the contradictions have been published and nothing that tends to do more than emphasise the point that inflammatory are often superimposed upon the meiotic phenomena. The evidence against the occurrence of specialised forms of chromosomes is equally unsatisfactory.

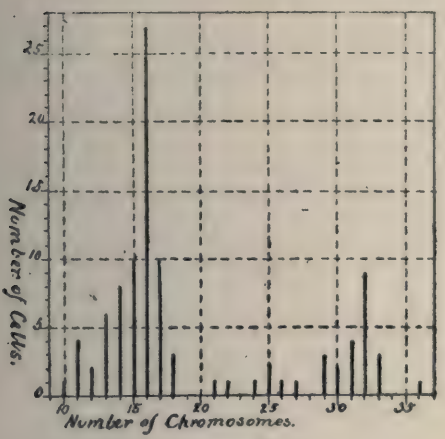
The only cells in the body of any multicellular organism, whether plant or animal, in which the symmetrical reduction in the chromosomes to half the somatic number takes place as a physiological (normal) process, are the gametogenic cells and these gametogenic cells are the only cells that normally pass out of somatic co-ordination. Therefore, apart from any other and perhaps equally important though less easily demonstrated points of resemblance, the suggestion is very strong indeed that the meiotic phenomena are due to the cells having passed out of somatic co-ordination. The gametogenic cells[4] differ but little

[1] From Walker and Wittingham, *op. cit.*

[2] Walker and Whittingham, *op. cit.*

[3] Farmer, Moore and Walker, *Proc. Roy. Soc.* B, vol. lxxvii. 1906.

[4] The cells whose descendants are destined to produce gametes. This term obviously excludes the mature gametes.



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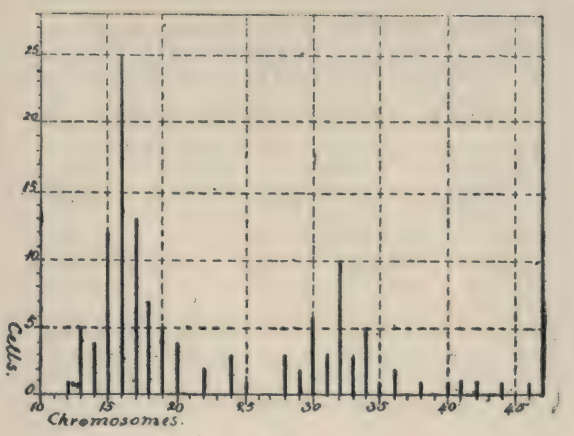

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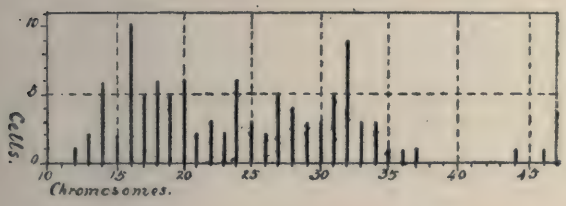

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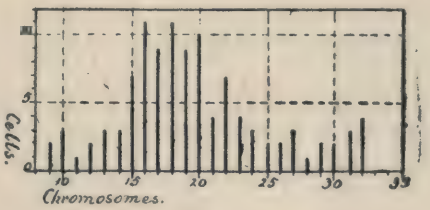

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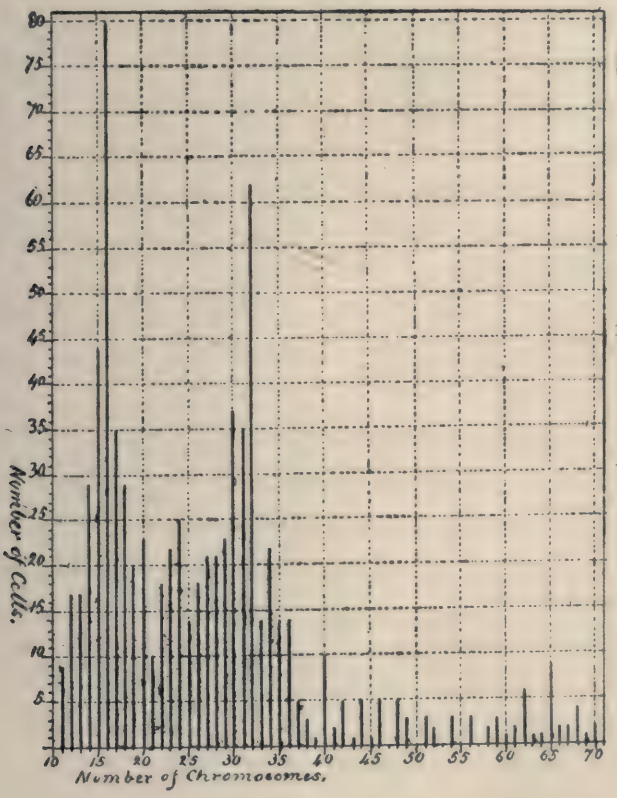

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in essentials from the early blastomeres and the process of reduction involves no actual specialisation but rather a capacity

of exhibiting a particular phenomenon. There seems nothing improbable in the supposition that this potentiality—which is retained by the gametogenic cells through many generations to be exhibited only upon a particular occasion and probably only under certain conditions—may be retained by other cells of the body, though no physiological set of conditions can appear to bring it into action. Such a supposition implies no more than that in all the cells of the body in which differentiation has not advanced to a stage which precludes further multiplication the potentiality is retained of going so far on the road towards the production of gametes as is involved in the meiotic phase. When this occurs in gametogenic tissue, that is physiologically, specialisation follows and the result is the production and throwing off of gametes. In other cells, however, though they go through the meiotic phase after passing out of somatic co-ordination as the result of chronic irritation or some other unknown stimuli, the environment is not such as allows of the production of gametes and the result is the production of a group of cells living in a parasitic manner upon the parent organism, cells which multiply indefinitely and eventually destroy their host.

As the gametogenic cells pass out of somatic co-ordination, so do the cells which form cancer; living as they do in a parasitic manner upon the parent organism, both must be regarded in a limited sense as independent individuals. The malignancy and the meiotic phenomena of cancer are both caused apparently by the cells having passed out of somatic co-ordination. It is not suggested that the malignancy is due to the cells having passed through the meiotic phase.

Many writers appear to think that the interpretation of this phenomenon involves the belief that the cells of cancer are of the same nature as the gametogenic cells. The mere fact that the term "gametoid"[1] is applied to the cancer cells shows that this is not the case. Meiosis (reduction) is obviously a very primitive phenomenon. It occurs universally among plants and animals in the production of gametes and in both gametogenic and cancer or gametoid cells it follows upon passing out of somatic co-ordination. As it does not appear among the cells which are in somatic co-ordination, there can be no suggestion that meiosis is the cause of this aberrance.

[1] Farmer, Moore and Walker, 1903, *op. cit.*

THE "INHERITANCE OF ACQUIRED CHARACTERS" IN TRYPANOSOMES

By EDWARD HINDLE, Ph.D.

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Owing to their great economic importance the Trypanosomes have been studied more closely than almost any other group of the Protozoa. The result has been that they are found to present many interesting biological features. One of the most important discoveries is that these parasites possess the power of responding rapidly to changes in external conditions and of acquiring new characters which may continue during an indefinite number of generations. Many papers have been published on this subject recently, most of them devoted to a consideration of the responses of trypanosomes to the action of various medicaments. As the articles have been printed, as a rule, in medical journals that are rather difficult of access to most biologists, the writer has thought it desirable to collect the more important results and to consider them from a general point of view.

(*a*) *The Production of Mono-nuclear Trypanosomes by the Action of Drugs.*—It is well known that the size of the kineto-nucleus in trypanosomes is subject to considerable variation according to the species and that it varies from the large rod-shaped kineto-nucleus of *T. lewisi*, the rat trypanosome, to the minute spherical body representing this structure in *T. equinum*, the cause of "Mal de Caderas." Werbitski,[1] however, was the first to notice that in some races of trypanosomes the kineto-nucleus, which is supposed by many writers to control the locomotor functions of the parasite, was altogether absent. On examining a number of races of *T. brucei* (Nagana) that had become resistant to the action of certain drugs, he discovered that, in the case

[1] Werbitski (1910), "Uber blepharoplastlose Trypanosomen," *Centralbl. f. Bakteriologie*, vol. liii. pp. 303–15.

of one race that had become resistant to pyronin, the kineto-nucleus was absent from every trypanosome, although both tropho-nucleus and kineto-nucleus were present and showed no abnormal features in the original race of *T. brucei* from which this pyronin-resistant race had been produced.

Werbitski then injected the original race of *T. brucei* into a mouse and when its blood was swarming with parasites injected a maximum dose of pyronin. On the following day 40 to 60 per cent. of the trypanosomes were without a kineto-nucleus. The next day their number was less and on inoculating this race into another mouse the trypanosomes without kineto-nuclei rapidly diminished and in the case of a third animal completely disappeared. It was found by experiment that the "mono-nuclear" trypanosomes were less resistant than the normal "bi-nuclear" forms and that in consequence they were crowded out by the latter. It was then found that substances other than pyronin possessed this property of causing the disappearance of the kineto-nucleus, *e.g.* representatives of the oxazine series and especially orthoquinonoid substances.

Certain preparations of oxazine, given in doses of $\frac{1}{1500}$ to $\frac{1}{2000}$ of the body-weight of the infected mouse, caused 80 to 90 per cent. of the trypanosomes to lose their kineto-nuclei within twenty-four hours. This race was passed through ten mice in succession, an injection of oxazine being given in each case; at the end of this treatment it was found that the kineto-nuclei had disappeared from all the trypanosomes. This race of mono-nuclear trypanosomes maintained its peculiarity through several generations, although none of the animal hosts through which it was passed were treated with any drug.

The mono-nuclear trypanosomes do not differ in any other respect from the ordinary bi-nuclear forms from which they have been derived. In both races the same variations occur in the shape of the parasites—such as the long and narrow, short and broad, division forms, etc. The flagellum, in both cases ends in a small club-shaped thickening known as the end-bead, which is considered by Prowazek to be the equivalent of the centriole. This end-bead is usually situated in close relation to the kineto-nucleus and the latter is commonly supposed to control the movements of the flagellum through the end-bead. There is no difference in motility, however, between the mono-

nuclear and bi-nuclear trypanosomes; therefore the power of movement cannot depend on the kineto-nucleus. This name, accordingly, becomes a little inappropriate but it is advisable to retain the term, as it is now generally used to denote the smaller and more densely staining nucleus of trypanosomes. This is an excellent example of the danger of attributing functions to organs on purely morphological grounds, nearly every writer having assumed that the kineto-nucleus was the locomotor centre of the trypanosome.

Werbitski conducted experiments to ascertain how long a mono-nuclear race of trypanosomes would persist when living, generation after generation, in normal mice. In the first series the blood of the fifteenth successively infected mouse showed only mono-nuclear trypanosomes; in the sixteenth mouse a few parasites were found in which the kineto-nucleus had reappeared; after a few more generations (passage through six to eight mice) the race had become indistinguishable from the normal bi-nucleate type. But this *apparently* normal race possessed a very striking peculiarity: when exposed to the action of substances having no effect on the morphology of normal trypanosomes, the kineto-nucleus again disappeared. For example, acetylatoxyl, arsacetin and tryparosan—substances which have no effect on the nuclei of normal trypanosomes—all caused the disappearance of the kineto-nuclei from the recovered race. The newly acquired kineto-nucleus is thus shown to be more labile than the normal nucleus.

In some cases the mono-nuclear race of trypanosomes preserves its peculiarity, without any further treatment, during an indefinite period. Laveran[1] found that a race of *T. brucei* that had been exposed to the action of pyronin was still mono-nucleate after having passed through 130 animals successively (a period extending over more than one year).

There are two possible explanations of the origin of the mono-nuclear races of trypanosomes. Kudicke[2] having found that a small percentage of trypanosomes that had never been exposed to the action of any drug did not possess a kineto-nucleus, it has been suggested that these mono-nuclear races

[1] Laveran (1911), "Contribution à l'Étude du *Trypanosoma brucei* sans Blépharoplaste de Werbitski," *Bull. Soc. Path. Exot.* 1911, pp. 233–9.

[2] Kudicke (1911), "Die Wirkung orthochinoider Substanzen auf Rattentrypanosomen," *Centralbl. f. Bakteriologie*, vol. lix. pp. 182–5.

arise merely by the selective elimination of the bi-nuclear forms. This explanation, however, is proved to be erroneous by the fact that it is possible to cause the disappearance of the kineto-nucleus in trypanosomes that have ceased to multiply. For example, when *T. lewisi* has reached the chronic stage of infection in the rat, all multiplication ceases and yet it is possible by appropriate drug treatment to cause the disappearance of the kineto-nuclei from these parasites. The drug is thus shown to act *directly* upon the kineto-nucleus and not by any process of selection; it is extremely probable that all drug-resistant races of trypanosomes arise in the same manner.

Laveran and Roudsky[1] have investigated the mechanism of the disappearance of the kineto-nucleus in trypanosomes that were exposed to the action of oxazine. This compound exercises a selective staining action on the living parasites, the kineto-nucleus being the only part that is coloured. After taking up the stain, the kineto-nucleus gradually diminishes in size and finally disappears, a process which is easily followed under the microscope. The two authors consider that the disappearance is due to the destruction of the nucleus as the result of a process of autoxidation caused by orthoquinonoid compounds. A strong confirmation of their hypothesis is the fact that minute quantities of potassium cyanide—and also other compounds possessing its power of preventing oxidation in the tissues—prevented the oxazine either staining the kineto-nucleus or causing its disappearance.

(*b*) *The Production of Virulent Races of Trypanosomes by Passage through Cold-blooded Animals.*[2]—Fellmer,[3] in 1908, was the first to report that the virulence of *T. brucei* and *T. lewisi* respectively could be altered by passing them through cold-blooded animals.

The original observations have been considerably extended in two later communications by Wendelstadt and Fellmer,[3] who show that the trypanosomes are not only altered in virulence

[1] Laveran and Roudsky (1911), "Au sujet de l'action de l'oxazine (chlorure de triaminophénazoxonium) et de l'akridine (diphényl méthane) sur les trypanosomes," *Compt. Rend. Ac. Sci.* No. 20, pp. 916–9.

[2] It should be added that Wendelstadt and Fellmer's results have not yet been confirmed by other workers.

[3] Wendelstadt and Fellmer (1909), "Einwirkung von Kaltblüter-passagen auf Nagana- und *Lewisi*- Trypanosomen," *Zeitschr. f. Immunitätsforschung*, August, pp. 422–432.

but also in form. Their method of procedure was as follows: The cold-blooded animals were inoculated in the peritoneum with a small quantity of undiluted blood containing the trypanosomes under investigation. After an interval of a few days some of the blood of the inoculated cold-blooded animal was injected into the peritoneal cavity of a normal rat. The rat frequently became infected with a modified form of the original race of trypanosomes and by infecting other animals from this rat the virulence of the parasites towards their host could be enormously increased. A large variety of cold-blooded animals were found to harbour infection, *e.g.* grass-snakes (*Tropidonotus natrix*), lizards, tortoises and even beetles and molluscs.

The most interesting results were obtained by the passage of *T. lewisi* through grass-snakes. This common species of trypanosome is not known to exercise any harmful effect on the rats in which it lives (about 30 per cent. of all rats are naturally infected with it) and consequently it has always been regarded as one of the non-pathogenic forms. Moreover, ordinary races of *T. lewisi* will not live in the blood of any other mammal except the rat, the normal host. When the blood of a rat containing many trypanosomes (*T. lewisi*) is inoculated into a grass-snake the latter often becomes infected, even though the parasites may never actually be seen in the infected snake. Their presence can only be detected by inoculating some of the snake's blood into a healthy rat, which subsequently may become infected with a modified race of *T. lewisi.* The most remarkable of the modifications is an alteration in virulence which, however, is not usually apparent in the rat infected directly from the snake but only after inoculating other rats from the first one. After passing through a few rats in this way, the new race becomes pathogenic towards its hosts and when injected into other rats invariably kills them after a short period of incubation. One of these new virulent races of *T. lewisi* killed rats only four days after they had been inoculated with it, the incubation period (*i.e.* the time elapsing between the inoculation and the first appearance of parasites in the general circulation) being shortened from three or four days down to only twelve hours. These pathogenic races of *T. lewisi* maintained their peculiar characters through many generations of rats without showing any diminution in virulence. It was also found

possible to infect other species of mammals, such as mice and guinea-pigs, with this new race—a further proof of its altered characters.

In addition to this alteration in pathogenic effects, the virulent race of *lewisi* presents certain morphological peculiarities distinguishing it from the normal forms. The most conspicuous feature is a great increase in size combined with marked granulation of the protoplasm. The virulent trypanosomes stain much more readily and appear darker than the normal form; the nucleus also is more irregular. The increase in size is very remarkable: under normal circumstances, the extreme length of the rat trypanosome is about 30μ but the augmented race contained individuals measuring as much as 40μ and even more. Also parasites may be found in which the posterior end is enormously drawn out into a long beak almost equal in length to the remainder of the body. This form has only once been described as occurring in normal races of *lewisi* but is comparatively common in the virulent strains.

The above-described results show clearly that both the pathogenic character and form of trypanosomes may undergo considerable modification when external conditions are varied; it may therefore be questioned whether in nature it be possible for harmless parasites to acquire harmful characters suddenly. In the case that we have described practically a new rat disease has been created, ordinary races of *T. lewisi* exercising no injurious effect upon their hosts, whereas the virulent form is highly pathogenic, soon killing the rats which become infected with it. These experiments seem to show that at the present time under suitable conditions new diseases may arise and it is not impossible that the recently described human trypanosome (*T. rhodesiense*), which is the cause of sleeping sickness in Rhodesia, may have arisen in modern times as the result of some modification of a previously harmless parasite.

(*c*) *Drug-resistant Trypanosomes.*—One of the more interesting and also the most thoroughly investigated of the responses of trypanosomes to changes in their environment is their power of becoming resistant to the action of drugs.

The existence of this property was discovered by Franke and Roehl[1] whilst working in Ehrlich's laboratory on the treat-

[1] *Vide* Ehrlich (1907), "Chemotherapeutische Trypanosomen-Studien," *Berl. klin. Wochenschr.* pp. 233–6; 280–3; 310–4; 341–4.

ment of infections of *T. brucei* by parafuchsin. These observers found that on feeding mice infected with *brucei* with parafuchsin the trypanosomes disappeared from the blood for a long time. Eventually, however, they reappeared in the circulation of the mouse but could be banished again by a second course of feeding with parafuchsin. But this alternation did not continue indefinitely, as the intervals between the reappearances (or "relapses") of the trypanosomes gradually became shorter until finally the drug had no effect on the parasites. In other words, a race of trypanosomes had been produced able to resist treatment with parafuchsin. Moreover, when a second mouse was infected with this race the trypanosomes still maintained their power of resisting the drug.

It was next found possible to develop races of trypanosomes resistant to other trypanocidal agents and thus the important fact was established that these parasites are able to acquire the power of resisting the effects of drugs.

Shortly after atoxyl was introduced as a trypanocidal agent, Browning produced in mice an atoxyl-resistant race of trypanosomes. This resisting power persists through a large number of generations, as long as the trypanosomes are kept in the species in which the property was developed. Thus one atoxyl-resistant race of *T. brucei* in mice still maintained its power of resistance after having passed through 140 mice during a period extending over fourteen months. In another case, however, the resistance, though still marked after the trypanosomes had passed through sixty-seven mice in six months, was not apparent about seven weeks later after the organisms had passed through eighty-nine mice.

It was subsequently demonstrated by Mesnil and Brimont[1] that atoxyl-resistant races of trypanosomes were only resistant in the species of animal in which this property had been developed. Their race of *T. evansi* maintained unimpaired its acquired power of resisting atoxyl even after 110 passages through mice without any fresh contact with the drug. Yet when this race was injected into white rats, the trypanosomes at once became susceptible to the action of atoxyl, all power of resistance having disappeared. When this race had been passed through 20 rats without becoming atoxyl-resistant, it

[1] Mesnil and Brimont (1908), "Sur les propriétés des races de Trypanosomes résistantes aux médicaments," *Ann. Inst. Pasteur*, vol. xxii. pp. 856-975.

was again put back into a mouse; the trypanosomes at once became atoxyl-resistant, showing that this character had been preserved though the parasites had not come into contact with the drug.

Breinl and Nierenstein[1] also described an interesting case in which they developed an atoxyl-resistant race of *T. equiperdum* (dourine) in donkeys. This race was inoculated into white rats and afterwards passed successively through guinea-pigs, rabbits and rats during a period extending over seven months; when finally a donkey was inoculated, it was found that the race had maintained its resistance to atoxyl after all this time.

It has since been shown that atoxyl does not act directly on the parasites but only after it has entered into combination with the proteids of the blood. Therefore, when a race has acquired the power of resisting the compound of atoxyl with mouse-proteid, it does not necessarily possess any power of resisting the compound of atoxyl with the serum proteids of another species as the blood of different animals is well known to differ. Consequently, the specific nature of the character of atoxyl-resistance is easily explained.

The experiments referred to have been described mainly with the object of showing how persistent a character may be in these parasites when once it has been acquired; and also to show how rapidly such an acquired character as that of drug-resistance may be produced. Finally, we come to the most interesting and important results that have been obtained from a study of this character of drug resistance, viz., those of Gonder.[2]

It had long been a contested point whether or not this character was maintained when the parasites were transmitted by the normal intermediate host. The majority of writers seem to have assumed that it was maintained but no experiments had been made on the subject until Gonder attempted to transmit a resistant race of *T. lewisi*, the rat trypanosome, by means of the intermediate host *Hæmatopinus spinulosus*, the rat louse.

The race of rat trypanosomes used was one that had acquired the power of resisting arsenophenylglycin and had retained this character after passage through a large number of rats.

[1] Breinl and Nierenstein (1908), "Weitere Beobachtungen über Atoxylfestigkeit der Trypanosomen," *Deutsche Med. Wochenschr.* vol. xxxiv. pp. 1181–2.

[2] Gonder (1911), "Untersuchungen über arzneifeste Mikroorganismen: I. *Trypanosoma lewisi*," *Centralbl. f. Bakteriologie*, vol. lxi. pp. 102–113.

Large quantities of lice were transferred from rats infected with this drug-resistant race to normal rats. The latter became infected in a certain number of cases and the result differed according to the mode of infection. In two cases the transmission was merely mechanical, *i.e.* the trypanosomes were carried on the mouth parts of the lice and directly inoculated into the new host. In both these cases *the drug-resisting power was maintained.* On the other hand, in the majority of cases the transmission was indirect, *i.e.* the trypanosomes passed through a developmental cycle in the intermediate host before the latter became infective. After this cycle had been passed through, it was found *that the acquired character of resisting the drug was completely lost.* Accordingly when rats were infected by these lice, the trypanosomes which developed in their blood were quite normal and an injection of arsenophenylglycin caused their disappearance from the circulation; the resistant race of trypanosomes kept as a control was quite unaffected by a similar injection.

In addition to these transmission experiments, Gonder attempted to ascertain at what period in the louse the trypanosomes lost their power of resisting the drug. For this purpose several hundred lice were taken from a rat infected with the resistant race. Each day about 50 to 100 of the lice were triturated in normal saline solution and the emulsion injected into healthy rats. The animals became infected with the trypanosomes after varying periods of incubation. They were then injected with arsenophenylglycin in order to ascertain whether or not the drug-resisting power had been maintained. The drug-resisting power was maintained during the first ten days after the lice had been taken from the rat; consequently, all the rats which received injections during this period became infected with the drug-resisting race of trypanosomes. Eleven and twelve days after the lice had been removed the drug-resisting power was noticeable in some of the infected rats but not in others, whereas *all rats* into which lice were injected that had been removed more than twelve days from the original animal became infected with a perfectly normal race of the rat-trypanosome. *The power of resisting the drug had disappeared completely* and as a result when each of these rats was injected with arsenophenylglycin the trypanosomes disappeared from its circulation.

These experiments clearly show that during the first ten days after being taken into the gut of the louse the trypanosomes maintain their acquired character of resisting the drug; from the tenth to the twelfth day some change takes place in the parasite which involves the disappearance of the acquired character and after this date the trypanosomes are quite normal.

Another interesting feature brought out by these results is the proof that the louse acts as a true intermediate host in the transmission of *Trypanosoma lewisi* from one rat to another.

Prowazek[1] has described a life-cycle of *T. lewisi* occurring in the *rat louse* and it is a noteworthy circumstance that he describes a sexual process taking place in the gut of the louse about ten days after the trypanosomes have been ingested. The date of the disappearance of the acquired character of drug-resistance and the appearance of the so-called sexual process of the trypanosome are thus identical; there can be no doubt that these results afford strong support to Prowazek's account of the life-cycle of *T. lewisi* in the intermediate host.

In addition to the above-described experiments, Gonder also attempted to decide whether the power of resistance was lost when the resistant race of trypanosomes was merely grown on culture media. It was found that trypanosomes which had been grown on blood-agar during a period of more than three months still remained drug-resistant. As a result, rats injected with these "cultured" trypanosomes became infected with a drug-resistant race of the parasites. The acquired character was not lost.

There is obviously a difference, therefore, between the changes that take place in the trypanosomes in the gut of the intermediate host and those occurring in culture tubes. The view of certain authors that the gut of the louse acts in the same way as a culture-tube is thus shown to be erroneous.

In conclusion, attention may be drawn to the especial interest these experiments have for those who believe that the sexual process involves an elimination of acquired characters. The trypanosomes that become drug-resistant *in direct response to an external influence* maintain this character through an indefinitely large number of generations, so long as the parasites are transmitted from one rat to the next by direct inoculation.

[1] Prowazek (1905), "Studien über Säugetiertrypanosomen," *Arb. a. d. kais. Gesundheitsamt.* vol. xxii. pp. 351–95.

After an acquired character has remained constant during several thousand generations, extending over a period of more than a year, one might easily assume that the character was fixed. Whilst in the blood of the rat, however, the trypanosomes only multiply asexually and therefore the character is not eliminated. After these trypanosomes have been taken into the gut of the intermediate host, in which the sexual cycle presumably takes place, the acquired character completely disappears and the parasites recover their normal behaviour towards the drug. As the acquired power of resisting a drug is eliminated by passing through the intermediate host, there seems little danger of arsenic-resistant races of *T. gambiense*, the cause of sleeping sickness, being spread by the tsetse-fly, its intermediate host.

REVIEWS

To the Editors, SCIENCE PROGRESS

IN asking for an opportunity of dealing with certain points of the review of my *Text-book of Inorganic Chemistry* in the January number of SCIENCE PROGRESS, I desire in the first instance to express my thanks for the liberal amount of space devoted to the consideration of my book. In so far as the criticisms express the personal opinions of the reviewer, I have no comments to offer, being content to leave the question of their justification to the judgment of others. The review contains, however, certain serious misstatements of fact and I request that the same prominence may be afforded to the corrections as was given to the original statements.

The criticism of the chemical part of the book is as follows:

". . . In dealing with the interaction of nitric acid and metals not one word is said of the work done by Russell, Veley and others showing the part that is played by nitrous compounds. Under 'Modern Methods of Writing Complicated Equations,' printed in Clarendon type, we have the following choice morsels:

"'An equation such as that representing the action of nitric acid on copper can readily be written in stages. In such a case it is perhaps simplest to assume that the oxygen of the nitric acid is used up in converting the metal to oxide, the latter then uniting with more nitric acid to form the nitrate. As a preliminary to writing the equation, the stage to which the nitric acid is reduced must be determined experimentally and the formula of the nitrate must be known. The steps in the action of nitric acid on copper are as follows:

(1) $2HNO_3 = H_2O + 2NO + 3O$
(2) $3Cu + 3O = 3CuO$
(3) $3CuO + 6HNO_3 = 3Cu(NO_3)_2 + 3H_2O$

Adding up—

$$3Cu + 8HNO_3 = 3Cu(NO_3)_2 + 2NO + 4H_2O.'$$

"What has the chemist to do with what 'is perhaps simplest to assume'? His duty is to ascertain, as far as possible, what actually happens; it is pretty certain that the picture here painted for us is purely imaginary."

So far the reviewer. I will now show that not one word of this criticism is justified.

The concluding paragraph of the section from which the above extract is taken is as follows (p. 227):

"An alternative method is to represent the first stage of the reaction as involving the liberation of hydrogen, which is then used up in reducing the nitric acid to a lower stage of oxidation. It is sometimes assumed that this represents the actual stages through which the reactions pass, but in reality they are probably much more complicated. The remarkable fact that pure dilute nitric acid has little or no action on copper or mercury unless a little nitrous acid is added cannot easily be accounted for on this view as to the mode in which the reaction proceeds."

The matter therefore stands thus:

(1) The whole bearing of the section is misrepresented in the review. It is evident from the parts quoted, especially the references to "an alternative method"

and the sentence beginning "It is sometimes assumed," that the partial equations represent a quasi-mathematical method of arriving at the full equation showing the action of nitric acid on copper and are in no sense meant as a picture of the actual course of the reaction. In this sense the use of the term "simplest assumption," criticised by the reviewer, is entirely justified.

(2) Contrary to the categorical statement in the review, the effect of nitrous acid in accelerating the action of nitric acid on copper (discovered by Millon) is mentioned in the book *in the same section as the extract given by the reviewer.*

(3) If the heading of the section had been "Modern Methods of Writing Complicated Equations" there might have been some little excuse for the assumption of the reviewer that the partial equations were put forward as representing the actual stages of the reaction. *As a matter of fact, the word "Modern" occurs only in the review*; the heading in the book is simply "Methods of Writing Complicated Equations."

In view of the above facts "the picture here painted for us" in the review is in very truth "purely imaginary"; it has no counterpart in the book.

It is the duty of a reviewer to make himself thoroughly acquainted with the statements of the author in order that no injustice may be done to his work and this is particularly necessary in the case of an unfavourable review. In this primary duty the reviewer has conspicuously failed.

GEORGE SENTER.

[I was aware of the belated reference to nitrous acid to which Dr. Senter calls attention and can only repeat—"not one word is said of the work done by Russell, Veley and others showing the part that is played by nitrous compounds." The reference is made in such a way that the student is none the wiser—in fact, this final paragraph of the section under notice can but serve to muddle him. "An alternative method" is mentioned only to be dismissed; being told that "it is perhaps simplest to assume," etc., having some slight sense of logic in his composition, the student will of course assimilate the "assumption" and offer it up as fact at the examination for which he is being crammed.

To teach in such a style is nothing short of a farce: "quasi-mathematical" it may be but it is not chemistry. We must either mend our ways of instructing youth or give up the attempt to teach science; most that is being taught is worse than useless. We advocate research and praise the researcher but shun him in the text-book.

I regret the insertion of the word "modern"; it was the survivor of a paragraph omitted in copying out the article.—H. E. A.]

The Great Star Map. By H. H. TURNER, F.R.S. [Pp. vi + 159.] (London: John Murray, 1912. Price 2*s.* 6*d.* net.)

THE four articles on the Great Star Map which Prof. Turner contributed to these pages are now reprinted with some additions in the form of a book. The author has remarkable skill in presenting difficult and technical researches in an easily readable form. No one, however unused to astronomical ideas, need fear to find this book too deep for him; yet it gives an insight into the aims and the practical methods of modern work which it has seldom been attempted to convey to the general reader. The great international project which gives the book its title has had a most profound influence on all branches of astronomical research to which photography has been applied and the subject is one which affords the author an opportunity for many interesting digressions into cognate subjects. Among these

side issues, the planet Eros, New Stars, Moving Clusters, Star Drifts and the absorption of light in space are all passed in review. Prof. Turner has produced a book which will certainly arouse interest in the important undertaking to which he has himself contributed so ably.

E.

Outlines of the Theory of Electro-Magnetism. A Series of Lectures delivered before the Calcutta University by G. T. WALKER, M.A., Sc.D., F.R.S. [Pp. 52.] (Cambridge University Press, 1910.)

THESE lectures were primarily intended to be of use to the lecturers in the outlying colleges and also to the more advanced students in Calcutta.

After a brief outline has been given of the methods of vector analysis, these are applied to various fundamental problems of magnetostatics and the electro-magnetic field from the Maxwell-Hertz point of view; the electron theory of Lorentz, both for fixed and moving media, is then developed by the same methods. The matter is necessarily highly condensed. There are one or two obvious slips, *e.g.* "Lemma 1" on p. 15 should read "Lemma 2," and "$Z_1 - Z_2$" on p. 21 should read "$X_1 - X_2$." On p. 34 the forces between two electric charges or between two poles are given as $\frac{Kee^1}{r^2}$ and $\frac{\mu mm^1}{r^2}$ instead of $\frac{ee^1}{Kr^2}$ and $\frac{mm^1}{\mu r^2}$ respectively, a very regrettable fundamental error.

G. W. O. HOWE.

Physical and Chemical Constants and some Mathematical Functions. By G. W. C. KAYE, B.A. (Cantab.), D.Sc. (Lond.) and T. H. LABY, B.A. (Cantab.) [Pp. vi + 153.] (Longmans, Green & Co. Price 4*s.* 6*d.* net.)

THIS book should satisfy a want that has long been felt for a set of Physical and Chemical Tables, in English, of convenient size and moderate price—something to be possessed, not consulted in a library. Opinions will always differ with regard to what should be selected from the ever-accumulating mass of physical and chemical numerical data for incorporation in a volume of this size. The data have been selected mainly with a view to their practical importance, though much has been included on account of its scientific interest rather than its utility. The chemistry section has received relatively somewhat scanty treatment, space being sometimes unnecessarily wasted in this section. For instance, formula weights might well have been omitted when formulæ and atomic weights are given. Nearly two pages are devoted to a table of factors for gravimetric analysis the only use of which is the saving of a little simple arithmetic. With one or two minor exceptions, however, the data have been most judiciously selected and well arranged. The authors have also dipped into such parts of astronomy, engineering and geology as border on physics and chemistry and have given considerable prominence to radioactivity and gaseous ionisation.

There is little doubt that the book will meet with a large demand. It is useful, trustworthy, up-to-date and inexpensive.

W.

Further Researches into Induced Cell-reproduction and Cancer. By H. C. ROSS, J. W. CROPPER, and E. H. ROSS. (London: John Murray, 1911.)

MR. ROSS has published a continuation of his investigations on cell-reproduction and cancer. He has extended the methods he has described previously, involving the use of "coefficient jelly," "auxetics" and "kinetics." We cannot

express conviction as to the validity of the results detailed nor do the figures which accompany them produce a more favourable impression. It is difficult to resist a feeling that the author and his collaborators are traversing difficult ground without an adequate acquaintance with the real nature of the dangers that must be inherent in any methods so drastic as those which they employ. In short, the appearances they describe are far more suggestive of lethal conditions than of anything else and having regard to the poisonous nature of the substances used in quite large doses, it does not seem necessary to regard them as due to any other cause. Nevertheless the influence of poisons on living cells is well worth investigating for its own sake but the results need to be very carefully used, as the factors involved are both numerous and hard to disentangle. At present, at any rate, it is premature to relate the results recorded by Mr. Ross and his colleagues with those cellular alterations responsible for the incidence of cancer.

Some Neglected Factors in Evolution. An Essay in Constructive Biology. By HENRY M. BERNARD. Edited by MATILDA BERNARD. (New York and London: Putnam's Sons, 1911.)

THOSE who enjoyed the personal friendship of the late Mr. Bernard will open the pages of a work which is largely posthumous with mixed feelings of pleasure and regret. Mr. Bernard was an ardent worker, as witness the years he devoted to the classification of corals. He was also a keen intellectual speculator and those who knew him will well remember the half-serious, half-humorous disputations on biological and sociological questions into which he could be so readily induced to enter.

The volume before us, which has been carefully and lovingly edited by his wife, who survives him, brings back to us very vividly the man as we knew him. Boldly speculating, often whimsical but always in earnest, he believed thoroughly in the message he felt called upon to deliver. In these "Neglected Factors" whoever runs may read his message—which enhances the evolution of life and only stops with its culmination in the highest development, mental and spiritual, of man. Probably many will hardly agree with the principles and conclusions enunciated in the book but that would not have surprised the author had he lived to see the completion of his work. At any rate the matter is well presented and Mrs. Bernard may be congratulated on the judicious care she has exercised in editing and preparing it for the press.

Bulletin of the Bureau of Agricultural Intelligence and of Plant Diseases. (International Institute of Agriculture, Rome.)

THE International Institute of Agriculture was established under an International Treaty of 1905 ratified by forty Governments and has since received the adhesion of eight others. The business of the Institute is to collect and publish as promptly as possible statistical and economic information concerning farming and agricultural products; to make known the new plant diseases that may appear in any part of the world and report on the spread of the disease and on effective remedies; to study questions concerning agricultural co-operation; lastly to publish various bulletins.

Of these latter, one contains abstracts of papers dealing with investigations directly or indirectly bearing on agriculture. The chief editor is Prof. Italo

Giglioli, who is aided by an able staff of abstractors. The quality of the work is excellent. The present number runs to over 360 pages and yet only contains abstracts of publications received at the Institute during one month. It is hardly necessary to say that no other publication deals in so full a manner with agricultural papers as this does. Hitherto the agricultural investigator, in this country at any rate, has had to depend on the American Experiment Station Record for his abstracts but although this has served a very useful purpose it is on nothing like the scale of the present publication. The Bulletin is published monthly in French and English at an annual subscription of 18 francs and deserves the cordial support of all who are in any way interested in agriculture.

E. J. R.

The Chemistry of Breadmaking. By J. GRANT, M.Sc.Tech. [Pp. vi + 224.] (London: Edward Arnold. Price 5*s*. net.)

WE agree with Mr. Grant that earnest students of breadmaking do require a book, that is reasonable in size and price, as an introduction to the subject. In many respects his work may be accepted as meeting this want. The writer has not quite freed himself from the trammels of that elementary science which ought to have been taught at school but his book is less open to this criticism than many similar technical works. There are plenty of books on elementary science available and it would have been far better to devote the space to breadmaking.

The author has in many places caught the spirit of his subject and writes as if he were acquainted with the modern work. The chapters on the cereals and milling are well done and contain some excellent illustrations. The description of the various processes of breadmaking is clearly written but it fails to indicate exactly how the bread of the various districts of Britain is being made at the present time. The relation of the flours from different parts of the world to the quality of the bread might well have been discussed more fully. A final chapter deals with the analysis of cereal foods. In scope the work is comprehensive and it may be recommended as a valuable addition to the literature of breadmaking.

E. F. A.

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